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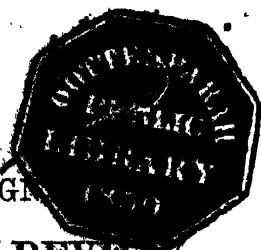
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THE

BRITISH AND FOREIGN



MEDICO-CHIRURGICAL REVIEW.

APRIL, 1855.

PART FIRST.

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AN eloquent living author, writing on "the history in words," prefaces his illustrations of such history by the following remarks:

"It might at first sight appear as if language—apart, that is, from literature and books, and where these did not exist—was the frailest, the most untrustworthy of all the vehicles of knowledge, and that most likely to betray its charge: yet it is, in fact, the great, oftentimes the only, connecting link between the present and the remotest past, an ark riding above waterfloods that have swept away every other landmark and memorial of ages and generations. Far beyond all written records in a language, the language itself stretches back and offers itself for our investigation—the pedigree of nations,' as Johnson calls it—itsself a far more ancient monument and document than any writing which employs it."*

The history of a science is often written in the names by which, through successive ages, it has been known, and in the nomenclature which it employs to designate the objects of which it treats. It is not our intention to pursue this thought with regard to physiology further than to recal the originally wide signification of that term—the discourse, science, history, or "word" of nature. Unrestricted as it is now to the phenomena of organized beings, the science of physiology, among the ancient philosophers, represented the sum of their knowledge with regard to all the varied manifestations of what they conceived to be an all-pervading life, revealing its power and ceaseless energy in earth, air, fire, and water, as well as in the organized creatures which might make those so-called "elements" their home. Gradually, the limits of physiology have been narrowed, and in the present day we include within its circle only the phenomena and laws of life as they are displayed by organic bodies. It is our purpose, in the present article, to inquire into some of the reasons which have led to these progressive separations; briefly reviewing the history of our science, we shall endeavour to discriminate between the generic ideas of life which have prevailed at different and at the same periods of time; to point out what we conceive to be the proper position of physiology as a branch of human knowledge, its relation to the other sciences which surround it, and the method to be employed for its successful prosecution.

In the earliest ages of the world (so far as we can gather from the records which have been preserved), there was extensive acquaintance with the characters, mode of life, habits, and general utility of many animals. The immediate dependence of man upon the lower creation, both for food and for assistance in his daily toil, in his agricultural pursuits, and in the chase, would necessarily be the stimulus for increasing his knowledge, and, at the same time, a most important means to be employed in its pursuit. Some attempts at classification are to be found in these earliest records, but the physiological ideas which prevailed are obscured by their mixture with theologic notions and with religious feelings. The Indian and the Persian, seeing in the bodies of some animals a resting-place for the souls of their departed friends, and in others the foes of their protecting Deity, treated the one with reverence and the other with awe, and thus shut themselves out from the possibility of acquiring a knowledge which should dispel their superstitious dread, and confer upon them the benefit of its own practical utility. In Egypt, similar tendencies prevailed; the priest

* On the Study of Words, by R. C. Trench, B.D., p. 60.

was the depositary and source of all knowledge, and mysterious allegory was the medium through which some fragments were conveyed to others—or, rather, it was the veil which, attractive by its own charms, served to conceal from the general gaze, not only the truth, which each might have made useful to himself, but also the falsehood, which, apart from this veil of mythic beauty, would have appeared in its own naked ugliness, and repulsed the devotees both from his religious ceremony and his priest.

In Greece (as in Egypt), the various sciences were cultivated by the same person, and were all alike taught by him to the common people. The sage or philosopher was the naturalist, the moralist, and the physician. Having more truth to impart, and less error to conceal—his purpose being, primarily, instruction, and secondarily, power—he was more honest in his object, more successful in his work, and more enduring in his fame. The desire of the earliest Grecian philosophers was to reduce all forces and all forms of action to some one great general principle; and in their endeavours, to accomplish this end (with the exception of Hippocrates), they pursued the same *a priori* method, often losing themselves amid interminable labyrinths of thought, or wrecking their whole system, with many of its earlier observations, by striking upon some new and contradictory facts. Aristotle, the greatest of Grecian naturalists, combined his knowledge of zoology and physiology with other and almost universal knowledge and speculation. His genius was that of compilation and classification, but he often passed beyond these to the realization of abstract truths, and founded a school in which his disciples were the naturalists for many successive generations.

Pliny, Galen, Dioscorides, and others, made large additions to the sum of detail-knowledge; their works evince exhaustless powers of labour rather than the efforts of genius, and though they were of much importance to the science of natural history, they produced no material advance of physiology. During the Middle Ages, physiology shared the common fate of other branches of knowledge, being lost in the general degradation, or even intellectual death. The creation of universities awakened, or demonstrated that something had already awakened, the dormant spirit of inquiry, and once more earnestness and success were apparent in the search for truth. Philosophy ceased to be a branch of theological study, but still the different sciences were unseparated; the cultivator of one was the student of all; and thus the learned men of the fourteenth century resembled the early sages among the Greeks, differing from them, however, as M. Lodore G. St.-Hilaire has said, in exhibiting no efforts of invention and imagination, no aspiration towards the future, their eyes turned only towards the past, having no other thought than to study and comprehend the ancient masters, and gathering from their books to reconstruct the science of antiquity. In such an epoch, the supreme merit was erudition, and compilation was the prominent feature of European work. This continued until the sixteenth century, when men arose who added to their laborious investigation of the past not only analysis and criticism, but original observation and induction. Bernard Palissy, for example, recognised in fossil organic bodies the remains of previously living creatures (already noticed by Avicenna and others), and he did more than this, in rescuing them from the domain of *lusus naturee*, by inferring the ancient

submersion of the earth. The names of Vesalius, Fallopius, Eustachius, and others of this period, have been handed down to the present day by our daily recognition of their anatomical discoveries; and it is worthy of notice that the naturalists of the sixteenth century, being the practical physicians and surgeons of the time, directed their attention mainly to such lines of investigation as should prove of service in the science and art of therapeutics.

Though belonging to the history of chemistry rather than of physiology, the name of Paracelsus must not be omitted from this sketch of the sixteenth century. His preposterous pretensions and professorial arrogance, inaugurated by burning publicly in his class-room the works of Galen and Avicenna, gained many adherents to his tenets, the scope of which we shall endeavour to point out in the sequel, as they were subsequently developed and modified by Van Helmont and Ernest Stahl. It is sufficient for our present purpose to designate these men as the founders of the iatro-chemic school, who attempted to explain the phenomena of life in health and disease by variations in chemical processes of the body, under the guidance of a distinct spiritual entity, the "Archæus" having its throne on the cardiac orifice of the stomach. The illustrious Sylvius carried some of these notions to their extreme point, and the tendency of the time can be explained only by the vast accessions which were made to chemical science, and the propensity which exists in the human mind to seek for unity in its explanation of the various phenomena with which it has to deal—that "*idola tribus*," in accordance with which "the human understanding, from its peculiar nature, easily supposes a greater degree of order and equality in things than it really finds, and will invent parallels, and conjugates, and relations, where no such thing is."

More correct methods were adopted by others, and with a success which has been the germ of much now taking the rank of scientific truth. Clusius (or Charles de l'Ecluse), by introducing into his classes the animals and plants of other countries, endeavoured to make natural history universal and comparative. Rondelet and Belon were the creators of ichthyology, the latter placing side by side the skeletons of man and of the fish, pointing out their analogous parts, and thus hinting at the "unity of composition" in organic bodies, which it was the honour of Geoffroy St-Hilaire to demonstrate. Conrad Gesner, at whose prodigious labours of compilation we can only wonder, laid the foundation, by indicating the true principles of its formation, of the "natural system" in botany. Cæsalpinus spoke confidently of the circulation of the blood, and not only of the lesser (pulmonary) circuit already known to Servetus, but of the larger (systemic) current. Fabricius pointed out the existence and direction of the venous valves, as well as the transition stages of the embryo; while his pupil Harvey demonstrated the circulation of the blood, and proclaimed the analogy between those transitory conditions of the fœtus and the permanent characters of the inferior animals.

The influence of Bacon and Descartes was now felt in all branches of scientific investigation; and although the attempts made by the latter and his followers to give mathematical precision and physical explanation to the phenomena of life (which transcend these methods), led to erroneous conceptions, and the most extreme divergence from the paths which

could conduct towards truth, the necessity for minute and careful observation, and for its no less faithful record, was increasingly felt by those who endeavoured to become "the interpreters of nature." The microscope was the great means of progress, under the direction of Leuwenhoeek, Hartsoecker, Malpighi and Swammerdam. Investigation of objects hitherto hidden, and its results, new and increasing knowledge, gave rise to doubts, and more than doubts, with regard to the correctness of ancient writers. The trammels of antiquity were thrown aside; and criticism, based upon original observation, became the *melior via* between the extremes of scepticism on the one hand, and reverence for traditional authority on the other. Minute observation was the necessity of the time, and for its successful prosecution the work was divided. Thus the seventeenth century prepared the way for those two illustrious men whose names will last, and whose memory will be revered, so long as the phenomena and laws of life form the subject-matter of human investigation. M. Isidore G. St.-Hilaire, to whose recent treatise we are indebted for many of the preceding facts, describes in such eloquent language the characteristics of Buffon and Linnæus, that we are sure the following quotation cannot but afford pleasure to the reader:

"Linné et Buffon sont nés précisément dans la même année, et à quatre mois seulement de distance, l'un en Mai, l'autre en Septembre, 1707; mais cette presque identité de dates, la puissance de leur génie, la grandeur des services qu'ils ont rendus à l'histoire naturelle, sont les seules similitudes réelles que l'on puisse signaler entre eux. Linné naquit pauvre dans un petit village de la Suède guerrière et encore barbare de Charles XII; Buffon, au sein d'une noble et riche famille, dans cette France que le règne de Louis XIV. venait de faire si grande. Linné, contraint au instant de se mettre en apprentissage chez un ouvrier, eut à soutenir une longue et pénible lutte contre l'adversité: si Buffon eut besoin d'une ferme volonté, ce fut pour résister aux séductions de cette vie molle et oisive dont sa fortune et son rang lui offraient le privilège. Tous deux enfin avaient reçu de la nature des tendances intellectuelles plus diverses encore que les circonstances au milieu desquelles ils durent se développer; il fut dans leur destinée de se compléter l'un l'autre par l'opposition des qualités contraires, et de s'estimer sans se comprendre. Linné, aussi patient, aussi sagace dans la recherche des faits, qu'ingénieux à les coordonner; plus prudent encore que hardi dans ses déductions; ne dédaignant pas de se tenir longtemps terre à terre, perdu en apparence au milieu d'innombrables détails, pour s'élever ensuite d'un vol plus sûr vers les hautes régions de la science; habile à former hypothèses, mais ne se faisant pas illusion sur elles, et lors même qu'il les étend à l'ensemble de la création terrestre, ne se laissant pas éblouir par leur grandeur; assignant, avec une étonnante sûreté de jugement, à chaque notion son rang et sa valeur, comme à chaque être sa place; doué d'une persévérance qui ne fut jamais ni découragée par les obstacles ni fatiguée par le temps; aimant la vérité pour elle-même, et trouvant que son expression la plus brève et la plus simple est aussi la plus belle; recherchant surtout dans son exposition cette élégance propre aux écrits scientifiques, qui résulte de l'enchaînement des pensées plus que du choix des mots; enfin, sans cesser jamais d'être exact et concis, variant son style depuis la précision austère de la formule jusqu'à cette haute poésie dont la Genèse nous offre des plus sublimes modèles: Buffon, sagace, ingénieux à l'égal de Linné, mais dans un autre ordre d'idées; dédaignant les détails techniques, négligeant de multiplier autour de lui les faits d'observation, mais saisissant les conséquences les plus cachées de ceux qu'il possède, et sur une base fragile élevant hardiment un édifice durable, dont lui seul et la postérité concevront le gigantesque plan; se refusant à emprisonner sa riche imagination dans le cercle étroit des méthodes, et cependant, par

une heureuse contradiction, créant un jour une classification que Linné même put lui envier; s'égayant parfois dans ces espaces inconnus où il s'élance sans guide, mais sachant rendre fructueuses ses erreurs même; passionné pour tout ce qui est beau, pour tout ce qui est grand, et s'il ne termine rien, osant du moins tout commencer; avide de contempler la nature dans son ensemble, et appelant à son aide, pour la peindre dignement, les trésors d'une éloquence que nulle autre n'a surpassée: Linné, un de ces types si rares de la perfection de l'intelligence humaine, où la synthèse et l'analyse se complètent dans un juste équilibre, et se fécondent l'une l'autre: Buffon, un de ces hommes puissants par la synthèse, qui, franchissant d'un pied hardi les limites de leur époque, s'engagent seuls dans les voies nouvelles, et s'avancent vers les siècles futurs en tenant tout de leur génie, comme un conquérant de son épée!" (p. 71.)

The 'Systema Naturæ' of Linnæus accomplished three most important objects. First, the establishment of a binary nomenclature for organized bodies. This, although common among the earlier naturalists, was not employed with any system to denote, as Linnæus made it, the persistence of generic similitude in conjunction with specific difference. Secondly, Linnæus subjected scientific language to invariable rules; and thirdly, he arranged and classified both animals and plants according to a new and comprehensive plan. Reaping, as we do, the fruit of his labours, it is not easy for us fully to appreciate all their value, since none but himself can know the hours and years of toil devoted to the cultivation of so vast and so rich a field.

The 'Histoire Naturelle' of Buffon was a daring and grand attempt to convey the philosophy of zoology. Its author perceived the unity of plan amid the endless diversities of organic development; he traced the laws regulating the geographical distribution of living beings, their successive appearance on the surface of our earth, and the limited variability of species. The analytic labours of Linnæus, and the synthetic work of Buffon, concurred to raise Natural History and Physiology to the rank of ordained sciences. Without the system of the former, and the power which it supplied for legitimate induction, it would have been impossible for the latter to have arrived at his most brilliant illustrations of induction. The two in combination present us with the method and the end of scientific investigation.

The close of the eighteenth and the commencement of the nineteenth centuries are crowded with the names and labours of men of whom the world is, with justice, proud. The names of Müller, Haller, Blumenbach, Spallanzani, Bonnet, Hunter, Duhamel, Humboldt, Cuvier, and Geoffroy St. Hilaire, do but feeble justice to the list,—they are familiar to us in their association with great discoveries of fact, or with the institution of great principles. Our space will not permit the separate consideration of their works, but we shall have occasion to refer to several of them in connexion with the next portion of our inquiry into the "ideas of Life" which have prevailed at different periods of physiologic history, and which are prevailing now.

It appears to us that we must arrive at some definite notion upon this subject—*what we mean by Life*—before we can consider to any profit the position which the study and science of its phenomena is to take in the encyclopædic scale, and before we can fairly judge of the method by which those phenomena may be reduced to scientific laws. Our own com-

plicated existence presents us with two classes or spheres of life for contemplation and study; and it is absolutely impossible for us, created as we are, to place in the same category the phenomena of life which we observe in our own material organs and in the animals and plants surrounding us, and those processes of thought and feeling of whose existence we are conscious, and even more certain than of the reality of an external world.* There appears to be so broad a line of distinction between that which knows and that which is known—between the intellectual or subjective, and the physical or objective life, that we may not lightly group their phenomena together, as being of the same kind and proceeding from the same source; although we are told, on the one hand, that all the processes of organic life are the result of an immaterial principle, the soul; and on the other, that all the flights of genius, and the “aspirations and sorrows” of the soul, are but the product of our material frame.

The views which, at different periods, have been entertained with regard to the nature of organic processes, have sometimes included those of intellectual life, at other times the two groups have been perfectly distinct; but although the particular notion held in respect of the one does not necessarily involve its application to the other, yet, as a general rule, that mode of considering either one which has obtained the most powerful hold upon the mind, has exerted some directive influence upon the course of ideas with regard to the other. It is our present intention to refer at first to the simplest phenomena,—those of organic life,—or such as the man shares, in common with the animal and vegetable creation generally, with the polyp and the lichen, and which separate him and them from the inorganic world.

There have been two generic ideas, irreconcilably opposed to each other, upon this point. According to one, Life is the principle or cause of organization and organic action; according to the other, its result, the sum of its actions, or its effect. Subject to endless modifications of expression, one or the other of these ideas may be found underlying the physiology of all times of which we retain the record. As we proceed, attention will be directed to a third method in which we may conceive of Life, but for the present we shall limit ourselves to these two.

First:—Life as a principle, or cause. This has been the prevailing notion under many different forms, of which we shall notice three:

1. The most ancient idea of Life was intimately connected with the belief that all matter, as such, was inert, and incapable of activity until receiving into itself some immaterial, energizing power. This power was Life: an emanation from the One Central and Divine Life, which, using the objects of the material world as its instruments, moulded them to its own will, or in accordance with the one comprehensive plan of the universe which it inspired. The “Life that glows in star and clod” was of the same essence as that which we feel acting in our own bodies; it was far higher and nobler than the organism in which it dwelt; and, indestructible by the accidents of time and place to which the latter might be subjected, it could pass through various phases of (objective) development, giving for a while power and energy of action to the living man, and then glowing with beauty in the flowers that blossomed on his grave. These notions were prevalent during only the infancy of science,

when (as we have already pointed out) all the different branches of knowledge were cultivated and taught by the same man—at one time the priest, and at another time the sage.

Physiologically considered, Life in this sense pre-existed and pre-determined both the organism and its form. It was the poetic myth out of which the science of Biology was to rise, rather than that science in a developed form. With some modifications, this is the idea of Life to be found in the second volume placed at the head of this article. Mr. Wilkinson has, with consummate ability and considerable eloquence, written what is rather a poem upon the human body than a system of physiology. Considered as the former, his work must command attention and admiration; if looked upon as a scientific treatise, our opinion must be very different. We can most satisfactorily represent his views by the quotation of his own words:

"That which is life in humanity, that which is life in society, that which is life in persons and in the moral soul, is also life, and the only life, in the organs of the frame. Ends, motives, passions, affections, likings, loves, virtues, are human vitality, and there is none other: . . . our being lies in cherished ends. . . . Remove these ends, and we stare without seeing, and sit in corners with hideous apathy and indecorum, miserably dishevelled and vegetalized; for life has nothing to do, and is taking its departure: as in metamorphosis, we are growing into trees, and the needy soil shall swallow us. Apply this to the body and its parts, and we find that the ends which it subserves in the order of things are its animating principles. They are not abstractions, but spirits embodied in works.

" We assume the soul, as also the existence of an imperishable humanity. It is a venerable creed, like a dawn or the peaks of thought, reddening their snows from the light of another sun,—the substance of immemorial religions, the comfort of brave simplicity, but the doubt of to-day, and the abyss of terrified science. . . . As the hand shapes the pen, and then writes with it, so the soul forms the body and then makes use of the properties resulting from the form. The connexion between the soul and the body is not more mysterious than the connexion between the pen-maker and the pen, excepting that our knowledge of the pen is so much more complete than our knowledge of the body." (p. 344 *et seq.*)

— Writing still farther upon the connexion of soul with body, Mr. Wilkinson remarks:

"We are apt, at first, to think that it is a single link or act; but this is an insufficient conception. There are as many different modes of connexion as there are wants in the soul, and organs, parts and particles in the body. There are as many different modes as there are possible species of contact in the great and the little creation. . . . This connexion of soul with body is no chaining of the living to the dead, like the horrid punishments of old times, but it is the live man freely working with the finest tools of nature—the chief musician in continual play upon the choicest instrument of music." (p. 346 *et seq.*)

It is not our intention to criticise separately the expressions contained in the above quotations, or to point out their self-contradiction. The passages have been introduced in illustration of one manner in which the phenomena of Life may be considered; partly with the intention of forming a strong contrast to that which we shall notice in the sequel, and partly because Mr. Wilkinson is the most recent exponent of a mode of thought common among the naturalists of antiquity, but rarely met with in the present day.

This idea of Life involves either an egregious error, or a *petitio principii*:

we recognise some forms of action in all the objects of the material world, from the movements of our own limbs and the unfolding of flowers, to the direction of the magnetic needle and the simple falling of a stone; but until it can be shown that all these varied phenomena are the results of one and the same force, we do wrong in calling them by the same name. If we apply the word *Life* to designate activity of all kinds, we are at liberty to do so, and Paracelsus had equal right and reason to call the medicinal virtue of substances "their stars;" but we shall then have to invent some other terms to denote those processes of organic bodies which we now call vital. The question, "What is Life?" remains precisely where we found it, if we merely say that it is a special and irreducible form of action; but the question is answered incorrectly if we affirm, in the present state of science, that it is one of many forms of action, all of which are due to the working of the self-same force. The interdependence of equilibrium and motion among the several physical forces indicates their correlation, but it by no means demonstrates their identity; and the processes of organic life, as observed in the animal and the plant, are no less separable from them than from anything to which we can reasonably apply the expression—*Soul*.

2. Life has been held to be a distinct entity, or principle, residing in the organism, and peculiar to each class and individual; pre-existent to the development of the body, and compelling the various materials of which it is composed into accordance with its own will, or in conformity with some general but unknown laws. Paracelsus was the inaugurator of this idea, differing from that of the ancient sages in recognising the distinctness of this imaginary being from the general soul in Nature. Van Helmont and Stahl systematized this conception of the *Archæus*, and described its seat, clearly indicating that the body was created and maintained by and for its pleasure; and that without its presence and Will the body could do nothing, feel nothing, would be dead. Under various names this idea was dominant for a long period in the history of *Phlebology*; at length the *Archæus* lost its individuality, and "animal spirits" numberless arose to take its place; a transition reminding us of the sailor's idea upon the origin of stars.

Doubtless many of the physiologists and physicians who made such constant use of the terms "animal and vital spirits," attached to such phraseology no notion of distinct entities, possessing wills and ways of their own; the words were merely used to express what we now mean by vital forces or vital properties: but others did not thus separate themselves from traditional authority, and the most extravagant explanations were given of the phenomena of health and disease, by their reference to the caprices and idiosyncrasies of these metaphysical creations.

In the present day we are by no means free from the same tendency of mind; and although we do not use precisely similar terms, nor speak of the animal spirits rising in wrath and contending with each other, we frequently hear of the "vital principle," and the "*vis medicatrix naturæ*," a phraseology which implies that these expressions are not unmixed with the idea of personality and discrimination in such abstractions. This arises very naturally from our tendency to link cause and effect in the world around us, as we find them linked in the world within, by the

tions qui résistent à la mort," is very severely criticized by the author we are considering; he finds in the expression no "other meaning than that life consists in being able to live." There is, however, in Bichat's too much extolled and too-harshly criticized definition more than the *argumentum in circulo*; there is the idea of "resistance" to that (death) which is disintegration, or the absence of a tendency to individuation, or the tendency overcome by more simple and more powerful affinities. The ideas of resistance to disintegration and of tendency to individuation (each necessarily implying the existence of the other) not so widely separated as they may appear at first sight, nor can we discover, as the author of the latter expression does, its vast superiority to the former.

We pass now to the second mode of considering life—viz., as the result, effect, or product of material properties and conditions. This view has been held in very different forms, but there are two generic ideas as their basis—one being, that vital phenomena are but the modification of more widely-distributed physical forces; the other, that they are absolutely irreducible either to physics or chemistry, and must be considered as the special product of a special structure—the organic body.

1. The chemical explanation of life dates back to the time of Paracelsus, who conceived first the application of chemical changes to unravel the mystery of organic processes. We have already seen that Paracelsus and his followers, though referring thus the phenomena of life to their favourite science, imagined all vital processes to be under the direction and control of a spiritual entity, the Archæus; but this idea of the Archæus was lost in the progress of time, and chemical changes of fermentation and decomposition were held to account for all the functions of vitality. Sylvius systematized these conceptions, and both he and his successors carried their speculations to the very lowest depths of absurdity.

2. Mechanical explanation of vital processes was attempted by Descartes, and the disciples of his teaching, in their endeavours to afford mathematical precision and certitude to all branches of knowledge,—Boerhaave, Sauvages, and other distinguished men, followed in this track; the most preposterous conclusions were arrived at, and confusion and disappointment were the result. According to the teachers of this class, digestion was simply a process of trituration; the circulating organs constituted an hydraulic machine, of which the heart was a suction-pump; and with complicated mathematical formulae, the force of the heart was shown to equal 180,000 lbs.!

3. The third mode of viewing vitality is, considering it to be merely a modification of electricity. This idea is only of recent date, since the remarkable discoveries made at the close of the last century. The readiness with which electric stimulation may become the occasion of either muscular contraction or sensation, led many hastily to arrive at the conclusion, that electricity and the nerve-force (meaning by that vital force) were identical. Most ingenious and elaborate treatises have been written to prove this point, but the phenomena of electricity in the living organism are now being viewed from a different stand-point, and we are becoming acquainted with their laws.

The three forms which have successively represented the first generic

idea of life as a result of material conditions have been assumed, as one or the other physical science has made the most rapid progress and has absorbed the attention of the day. Their error is the same—the confusion of correlation with identity. The organism is a physical body, and as such is subjected to physical laws, and is the medium for displaying the action of physical forces; but it possesses and displays others for which these (physical forces) can render no account. Chemistry, electricity, and mechanics may express some of the results of vital action; they are, at the same time, the instruments and the materials employed, but they by no means explain the process, nor can they convey all the results. Vital actions and vital forces hold a relation to physical actions and physical forces similar to that which is maintained by the latter to each other. The position of a bar of iron is affected by the magnet; the attractive power of the latter may be developed from electricity or galvanism, the accompaniment of chemie change, but their properties cannot be reduced to one; the disturbance of chemie equilibrium may be displayed by physical change, by heat, light, electricity, or magnetism, but the latter are not the products of the former. So with vital action: it may be called into exercise by any of the above, it may display itself (become phenomenal) through their agency, but it is not to be considered, on the one hand, as their cause, or, on the other, as their effect. If we gain nothing by the assumption of a separable "vital principle," we lose much by the attempt to reduce vitality to physics. Vast as the additions are which are constantly made to our knowledge of the material changes taking place in the organic body, there is a line beyond which, in this field of investigation (as in every other) the inquiring mind may press, but it is only into darkness and deep mystery, from which it must return, baffled and humbled by a consciousness of the little that is known compared with the infinity which lies beyond its grasp.

The second generic idea has assumed another form: life has been considered to be the result of material organization, but vital actions have been regarded as the special characteristics and properties of a particular structure—the organized body. It has been said, and, we believe, with perfect truth, that these functions are utterly irreducible to any physical forces, and that we must consider them as being equally distinct from the latter as they are from one another.

The recognition of vital properties may be said to have been hinted at by Glinson, but developed by Haller, and made the basis of a new era in physiology, under the able leadership of Barthez, Blumenbach, and Bichat. The expressions which have been used to define life according to this idea have been various. It has been denominated "a collection of phenomena in an organized being" (Richerand); "the organization in action" (Béclard); "the special activity of organized beings" (Dugès); "a double interior movement of composition and decomposition" (De Blainville); Bichat's definition we have already quoted—and these are sufficient to show the general similarity of idea: some expressing life as neither cause nor effect, but merely a special collocation of phenomena; others indicating its dependence upon the organism for its existence.

M. Auguste Comte, the founder of the "Positive School," adopts the definition of De Blainville, but adds that something more ought to be said

about the medium in which the organism is placed. In the third volume of his 'Positive Philosophy,' it is not easy to see that M. Comte clearly distinguishes between vitality and chemistry. In adopting the definition of De Blainville, life is considered as a complex interior process of composition and decomposition, and is thus made identical with its most general phenomenon. This, although in accordance with the whole system of M. Comte (inasmuch as it passes over the question "What is life?" in order to answer these two others—"What are its most general phenomena?" and "What are its laws?"), is unsatisfactory as a definition, and is open to the objection of being, at the same time, too inclusive and too exclusive. It would be difficult to apply the term "life" to every "double interior movement of composition and decomposition;" and it has no application at all to "irritability" and "sensibility," the special functions of animal vitality. In the 'Système de Politique Positive,' M. Comte clearly extricates himself from every suspicion of confounding vital phenomena with any other, and hurls somewhat angry invectives at those who have committed the mistake.

In the natural sciences (as usually understood) the method of M. Comte is that which has been followed, more or less closely, by all sincere investigators of truth since the time of Bacon and Descartes; but the theological and metaphysical beliefs which have mingled with the various branches of human knowledge are regarded by him as the modes in which the study of all sciences may be prosecuted, rather than any part of the sciences themselves. Thus he speaks of the theological and metaphysical "stages" as preparing the way for the "positive" final stage, in which the scientific man gives up all search after the causes, ends, and essences of things, as well as all belief in the existence of a Supreme Mind and Will, or in the existence of metaphysical entities, and seeks only to know the order of events and the sequence of phenomena. The deeply-lying question, Whether all our knowledge is, and ever must be, of phenomena alone? is not one the discussion of which would be appropriate in this place; but we shall endeavour in the next article to point out the manner in which M. Comte bridges over, by a "cerebral theory," the great gulf between the physical sciences and History, or the science of man's sociologic development.

With regard to the idea of Life as the result of organization and its media, we have only to observe that, although we have no acquaintance with it apart from them, they do not fill up the idea which is necessarily involved by its phenomena. The organism and the medium may be present, but the body may be dead, and neither micrologist nor chemist can tell us why. We have confined our attention mainly to those organic processes which man shares in common with the animal and the plant, and these are inexplicable as the result of the curious mechanism through and in which they are displayed; but the higher phenomena of animal and intellectual life transcend all references to their material embodiment, and lead us to another field of labour and another sphere for thought. The position, that life is the result of organization (says Coleridge) "seems little less strange than as if a man should say, that building, with all the included handicraft of plastering, sawing, planing, &c., were the offspring of the house; and that the mason and carpenter were the result of a suite

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of chambers, with the passages and staircases that lead to them. To make A the offspring of B, when the very existence of B, as B, presupposes the existence of A, is preposterous in the *literal* sense of the word, and a consummate instance of the *hysteron proteron* in logic."

We cannot, then, consider the vegetative life as either the cause or effect of organization, but as its special property, its vital endowment, differing in kind from every other, and utterly irreducible to the laws of ordinary physical force. We conceive that this vital property holds a relation to the organic body similar to that which subsists between magnetism and the magnetic needle. As we are unable to recognise magnetism without the magnet, although we may have the needle without the magnetic action, so we are unable to recognise life without the organism, although we may have the organism without life.

If the phenomena of vegetative life are irreducible to physical and chemic laws, those of intellectual and animal vitality are equally distinct from the processes of the nutritive sphere, although changes in the latter may be the mechanical means for their development. Our minds constantly seek for unity in their explanation of all the phenomena with which they have to deal; but until we can arrive at far higher and more complete realization of the truth which we feel is one, though revealed to us now in fragments which we often try and as often fail to adjust, we are in danger of placing incorrectly the partial knowledge we have gained.

(To be continued.)

J. Russell Reynolds.

REVIEW II.

On some Diseases of Women admitting of Surgical Treatment. By ISAAC BAKER BROWN, F.R.C.S.—London, 1854. 8vo. pp. 288.

MR. BROWN remarks in his preface, "There is no branch of surgery more open to improvement than that which relates to those accidents and diseases incident to the female sex which admit of no relief except from the hand of the surgeon." It may with equal truth be stated, that no branch of medical science has of late occupied the attention of the profession more than that relating to the female sex. Whether a proportionate amount of benefit has arisen out of such inquiries, and the proceedings resulting from them, is more debatable ground, and worthy of strict investigation. Advancement there has truly been in the number and kinds of manipulations practised upon the generative organs of females; and could our forefathers know what is being done in the present day, from the simple operation of applying caustic to the os uteri to the heroic proceeding of passing instruments into the Fallopian tubes, inclusive of all the various methods of dilating the os uteri and teaching the barren womb to cease from its barrenness and perform its proper childbearing function, they would be as much astonished at the march of intellect in the medical profession as at the wonderful operations and effects of steam conveyance and machinery. They would be at a loss to conjecture the probable consequences of the more active proceedings of their successors; and might entertain some apprehension, perhaps, lest the

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high tone of morality they so much admired in the weaker sex might not only not be enhanced, but perchance damaged, and a morbid desire for the investigation of "womb diseases" engendered. We are inclined to the opinion that even we ourselves might well pause to consider the effect of these innovations—to make out, if we can, how much good has resulted; what amount of injury, if any, has been inflicted, and how such injury may in future be prevented—to endeavour to ascertain whether womb diseases be in reality so frequent as would be inferred from the number of young women presenting themselves at our hospitals and dispensaries as the subjects of them, or whether they are not influenced in the description they give of their ailments by a morbid desire for the usual manipulations and examinations; whether, again, the natural desire of married females to have children does not lead them, under the prospects held out to them by "womb doctors," to submit to operations not only useless but sometimes positively injurious. Are such proceedings followed by the benefit they are stated to secure, or are they a form of professional quackery calculated seriously to injure the tone of public morals? We fear it must be acknowledged that whatever may have been the amount of good effected, a great deal of evil has also crept in; and the subject is highly deserving of the attentive consideration of every high-minded and conscientious practitioner.

In reviewing the work before us, however, we have not to deal with imaginary diseases, nor with operations admitting of question in a moral sense; but with stern realities—a class of diseases than which none can more embitter life, or be more difficult to cure; and the profession will feel themselves indebted to Mr. Brown for the energy and perseverance he has displayed in endeavouring to afford relief where the only alternative is misery.

The volume comprises thirteen chapters, under the following heads:—

1. Ruptured perineum; 2. Prolapse of the vagina; 3. Prolapse of the uterus; 4. Vesico-vaginal fistula; 5. Recto-vaginal fistula; 6. Lacerated vagina; 7. Polypus of the uterus; 8. Stone in the female bladder; 9. Vascular tumour of the meatus urinarius; 10. Imperforate hymen; 11. Encysted tumour of the labia; 12. Diseases of the rectum resulting from certain conditions of the uterus; 13. On ovarian dropsy.

Under the head of "Rupture of the Perineum," Mr. Brown describes an operation which he has many times performed with success, and produces eighteen cases, in all of which, one fatal one only excepted, a perfect cure of this troublesome accident was accomplished. It is difficult to arrive at a just conclusion as to the frequency of this occurrence, but our own experience would lead to the inference that it is very rare, and might be still more so with proper care and precaution. A careful attention to the generally accepted rules for supporting the perineum during labour will, in almost every instance, prevent the accident, or at all events limit it to an extent for which no other treatment than cleanliness and position will be required. It seldom takes place when the globular head of the child is the distending medium, unless the birth of the child be very forcible and sudden, and even then proper support will prevent it. The use of instruments is no doubt the most prolific source of ruptured perineum, and, as far as we have seen, the forceps now in such general use

(Assalini's) are the most likely instruments to produce it; they may be more easy of application, and more conveniently locked, than those recommended by Smellie, Davies, Blundell, and others; but they possess the disadvantage of having their handles widely separated from each other when applied, by which the perineum is put upon the stretch before the head comes down upon it, and is irregularly dilated by the forceps instead of uniformly by the round head of the child. We have witnessed the accident several times under the use of these instruments, and believe that forceps whose handles unite, as it were, in close contact with the presenting part of the head, and especially the vectis, are much less likely to occasion rupture of the perineum. It must, however, be admitted, that under any management whatever, rupture will sometimes occur, and in these rare cases the operation proposed by Mr. Brown appears to be very successful. We shall describe this operation in his own words, after quoting what our author says as to the means of preventing the accident:—

“Where rigidity of the perineum opposes the advance of the child, various remedies have been proposed to overcome it, as bloodletting, tartar emetic, warm fomentations, and greasy substances; but since the introduction of chloroform into practice I have never resorted to any of them, because I have found that in ten minutes, in the very worst cases, the parts have become dilatable when that agent is administered by inhalation.”

Mode of Operating.—“The patient should be placed in the position for lithotomy, the knees well bent back upon the abdomen, and all hair closely shaved off about the parts. The sides of the fissure should be held by an assistant, so as to ensure sufficient tension for the operator; a clean incision is now to be made about an inch external to the edges of and equal to the fissure in length, and sufficiently deep to reflect inwards the mucous membrane, and so to lay bare the surface as far as another incision on the inner margin. The denudation of the opposite side of the fissure is then to be practised in a similar manner, and the mucous membrane from any intermediate portion of the recto-vaginal septum to be also pared away. This denudation must be perfect, for the slightest remnant of mucous membrane will most certainly establish a fistulous opening when the rest of the surfaces have united. So soon as this stage of the operation is completed the sphincter ani is to be divided on both sides, about a quarter of an inch in front of its attachment to the os coccygis, by an incision carried outwards and backwards. The incision should be made by a blunt-pointed straight bistoury, which, having been introduced within the margin of the anus, guided by the forefinger of the left hand, is quickly and firmly carried through the fibres of the muscle and through the skin and subcutaneous areolar tissue to the extent of an inch, or even two, external to the anal orifice. The degree of relaxation to be sought must be regulated by the extent and character of the laceration; it being remembered, that the freer the incision the greater will be the amount of relaxation obtained. In every case muscular traction must be destroyed, for so long as it exists it will oppose the union of the parts.

“The sphincter having been divided in the manner just stated, the thighs are to be approximated, and then the quill sutures introduced. The left denuded surface and tissues external to it being firmly grasped between the forefinger and thumb of the left hand, a strong needle carrying a double thread is plunged, with the right hand, through the skin and subjacent tissue an inch external to the pared surface, and thrust downwards and inwards beneath it until its point reappears on the edge of that surface; it is then introduced at the corresponding margin of the denuded space of the opposite side, and made to traverse beneath it in a direction upwards and outwards until it escapes at a point equidistant from

the external margin with that at which it entered on the left side. Each of the three sutures is to be introduced in the same way, the one nearest the rectum first. The sutures are double, to allow them to enclose the quills, or (as actually used) the pieces of elastic catheter or bougie, around which they loop on one side and are tied over by their free ends on the other. For sutures I prefer stout twine, well waxed, to silk, as I believe it to be less irritating and productive of less suppuration.

"Having firmly secured the three sutures upon the bougies, the sides of the fissure become approximated—the denuded surfaces in apposition. To bring together the outer margins, along the line of the skin, it is advisable to pass three or four interrupted sutures. If this be carefully done, union of the skin will speedily take place, and that of the deeper parts be materially facilitated. I should recommend, previously to bringing the operation to a close, that the forefinger of the right hand should be passed into the vagina, and that of the left into the rectum, so as to ascertain that apposition is complete throughout. Lastly, the parts having been well cleansed by sponging with cold water, a piece of lint steeped in cold water is applied, and over it a napkin, kept *in situ* by a T bandage.

"The patient having been removed to her bed, should be placed on her left side on a water cushion, with the thighs and knees close together, and flexed on the abdomen. Perfect quiet enjoined, and cold-water dressing continued. Ice given to suck for twenty-four hours is refreshing, and allays febrile reaction and nausea. Two grains of opium should be given at once, and one grain repeated every four or six hours. Beef-tea and arrowroot may be given within the first twenty-four hours, but not wine, unless there are signs of flagging. After the first day, four ounces of port wine may be allowed, and a generous diet, chops, strong beef-tea, &c., after the second or third day. It is of great importance to draw off the urine by the catheter every four or six hours, for three or four or more days after the operation. On the eighth or ninth day, if the healing go on satisfactorily and the strength of the patient be equal to it, she may be allowed to pass water, resting on the hands and knees, so as to prevent, as far as possible, its contact with the lower or sutured surface of the vagina. The deep sutures should be removed on the third or fourth day in hospital patients, in private cases, on the fifth or sixth. I have found their retention after the periods named of no service, but rather mischievous, by their tendency to suppurate and slough—results of more rapid occurrence in hospitals than elsewhere, hence the earlier date proposed for their removal in hospital cases. On the sixth or seventh day the *external* sutures may be taken away—care must be taken not to separate the thighs, for it is necessary to keep up their apposition for some time. The opium should be persevered with, so as to keep the bowels constipated for two or three weeks after the parts have united. When union has become firm and complete, the bowels may be relieved by injections of warm water with castor oil, and by the latter given by the mouth. Attention should be paid during the passage of the first evacuation, and support given to the restored perineum if any hardened masses should cause stretching. Should adhesion, from any accident, not be complete throughout, and a fistulous opening persist, the actual cautery is the quickest and surest means of closing it: but the application of a caustic or stimulating substance may be tried." (pp 35 *et seq.*)

We have thus given our readers a detailed account of the operation as applied to cases of some standing, where cicatrization has occurred; with respect to recidive cases of the accident, the only variation of the plan is in the omission of the otherwise necessary denudation of the margins of the fissure. The chief novelty in Mr. Brown's operation is the division of the sphincter ani muscle, in lieu of Dieffenbach's elliptic incisions on either side the median line, to prevent too much stress upon the sutures.

In speaking of the surgical history of the subject, our author has not given sufficient credit to the Germans for their knowledge of this accident and its treatment. Siebold, in his '*Handbuch zur Erkenntnitz und Heilung der Frauenzimmerkrankheiten*,' dated 1826, gives a full description of the causes, degrees, effects, and treatment, of lacerated perineum. To promote the healing of the wound, in slight cases, he recommends the patient being placed upon her side, inclined more forwards than usual, in order that the lochia may flow anterior to the wound instead of over it, with the thighs tied close together. The urine to be passed kneeling. Clysters and aperients, if required, to prevent too much effort at stool; and great attention to cleanliness. If the rent extends to the sphincter ani, or through it, he advises an operation, consisting of paring the edges of the fissure as for hare lip, and uniting them by sutures, and if necessary, denuding also the recto-vaginal septum and including it and the external coat of the rectum itself in the ligatures. The bowels are to be well relieved and the diet regulated the day before the operation; and afterwards, one or two grains of opium are to be given, to prevent action of the bowels during the first few days. The after-treatment consists of keeping the patient several days upon her side in bed, with the thighs tied together, drawing off the urine frequently with the catheter, and giving meat broth and fluid nourishment. On the fourth day the wound is to be examined and cleaned, and caustic applied to any portion of the wound not found to be united.

In Busch and Moser's '*Handbuch*,'* under the head of "*Damm*," there is an elaborate article on the perineum, including an anatomical description of the parts, and the nature, causes, and effects of rupture, much in the same order as in Mr. Brown's work. The article occupies forty-five closely printed pages, and contains a great deal of information upon the subject. The various operations for the cure of rupture of the perineum are described at length, and a just comparison made of the different methods employed. The means of preventing the accident, and the after-treatment after operation, are also dwelt upon, and the literature of the subject discussed. No mention is made, however, of division of the sphincter ani muscle as a part of the operation; but when the parts are too tight, to prevent the risk of the sutures ulcerating through, the incisions of Dieffenbach are recommended. The plan of giving opium to prevent any action of the bowels for several days after the operation, is considered useless and even injurious, and the use of clysters of thin fluid to keep the rectum free, much preferred. We are disposed to think, from the perusal of Mr. Brown's reported cases, that, in some of them, less febrile and sympathetic disturbance would have followed the operation if the bowels had been carefully emptied by means of injections; and can scarcely understand why this should be more objectionable, if cautiously managed, than "injecting tepid water three or four times a-day into the vagina, to ensure cleanliness, and to prevent the irritation of the united surfaces by the lochia," especially after the resistance of the sphincter ani has been removed by the division of that muscle. But it is due to Mr. Brown to state that he makes a great point of keeping the bowels perfectly quiet, allowing no action; and considers that the opium

"proves actually beneficial by allaying irritation, by controlling inflammation, and by generally favouring the healing process."

Mr. Brown considers his method of treating ruptured perineum applicable in all instances advanced to the extreme degree; that the worst forms of laceration, of however long standing, may be cured by the operation; that immediately on the occurrence of the accident it should be resorted to; and that subsequent parturition is possible without injury to the restored perineum; and these several propositions are in a great degree established by the very interesting and successful cases he has detailed.

The next subject treated of is Prolapse of the Vagina, which, if it be of long standing, and occur in females beyond the period of child-bearing, is to be cured by an operation which will be sufficiently described in the history of the following case of vaginal cystocele:

"CASE.—M. J., aged 52, has had ten children. She was admitted in St. Mary's Hospital, Feb. 11, 1853, suffering from severe prolapsus of the vagina and bladder, which first began to trouble her nine years ago, after her last labour. On the least exertion of walking, or even standing, or coughing in the recumbent position, the tumour came down and protruded through the external orifice of the vagina, to the size of a large fist. On lifting up this tumour, when so extended, there were seen, on the under and posterior surface of the os uteri, which was dragged down by the vagina, two or three ulcerated spots, produced by friction against the posterior wall of the vagina. The patient could, when reclining on her back, replace the tumour. She had a cough from chronic bronchitis, which she generally had in winter, complained of feeling weak, and her appetite was capricious. This patient having been prepared, by emptying the bowels, was, on Feb. 15, placed under the influence of chloroform, and then put in the position for lithotomy, each leg being held by an assistant, a third assistant holding up the tumour with Jobert's bent speculum, and pressing it under the pubes in its natural position. A piece of mucous membrane, about an inch and a quarter long, and three quarters of an inch broad, was dissected off longitudinally, just within the lips of the vagina. The upper edge of the denuded part being on a level with the meatus urinarius, the edges were drawn together by three interrupted sutures, this being repeated on the other side of the vagina. The next stage of the operation consisted in dissecting off the mucous membrane, laterally and posteriorly, in the shape of a horse-shoe, the upper edge of the shoe commencing half an inch below the lateral points of denudation, taking care to remove all the mucous membrane up to the edge of the vagina where the skin joins it. Two deep sutures of twine were then introduced, about an inch from the margin of the left side of the vagina, and brought out at the inner edge of the denuded surface of the same side, and again introduced at the inner edge of the pared surface of the right side, and brought out an inch from its margin; thus bringing the two vascular surfaces together, which were then kept so by means of quills, as in the operation for ruptured perineum. The edges of the new perineum were lastly united by interrupted sutures, and the patient placed in bed on a water-cushion. Two grains of opium were given directly, and one grain every six hours; simple water-dressing applied to the parts; beef-tea and wine for diet. A bent metallic catheter, to which was attached an elastic bag to catch the urine, was introduced into the bladder; by this means the bladder was constantly kept empty. This patient progressed satisfactorily from day to day without a single bad symptom; and on the 22nd, the deep sutures were removed, and the parts were found firmly united. The lateral interrupted sutures were gradually removed, and firm union found to have resulted.

"Feb. 26. The deep union was perfectly sound, about three quarters of an inch thick, the lateral wounds well contracted; the tumour could not be brought down by coughing.

"March 8. The parts were all firmly healed; the patient was much improved in health, with a very cheerful aspect of countenance. She could walk about without inconvenience, and no amount of exertion produced any prolapsus. She could empty her bladder with comfort; and all the leucorrhœa discharge, which was so distressing before the operation, had entirely subsided; the offensive smell of the urine had also departed. On passing the finger into the vagina, the os uteri could be easily felt in its normal position, and the ulcerated spots which formerly existed on its surface were healed.

"On the 10th she was discharged, cured, and resumed her duties as domestic servant.

This was a very successful case; and the principle of this operation is said to be equally applicable to the cure of prolapse both of the posterior wall and of the entire circumference of the vagina; and also, with some slight modifications, to the relief of prolapsus uteri.

Vesico-vaginal Fistula.—On this subject but little new light is thrown by Mr. Brown's experience and practice; neither have his attempts at cure, although highly praiseworthy for the patience and perseverance they display in a good cause, been much more successful than those of others who have preceded him. His own views are well stated in his concluding remarks, in which he says—

"The preceding cases will illustrate the various points of difficulty which are met with in the treatment of this distressing lesion; and although they do not exhibit a great amount of success, they may fairly be looked upon as valuable illustrations of our present knowledge and practice; and I still look forward to a greater amount of success, by steady observation and persevering efforts, which the late improvements in surgical science certainly justify, especially as the difficulties are rather mechanical than pathological. It cannot be concealed however, that it requires no ordinary amount of perseverance and determination to bear up under the vexatious disappointments which are constantly occurring in the hands of the most painstaking operators."

The mode of operating preferred by Mr. Brown is, after paring the edges of the wound, to employ the form of suture used by Dr. Sims of Boston, U.S., called the "clamp" suture, a full description of which will be found at p. 93 *et seq.* of the work before us. Before taking leave of this subject, we cannot but express our concurrence in the following observations respecting the prevention of such a horrible calamity, borne out as they undoubtedly are by our own individual experience:

"As far as my experience goes," Mr. Brown observes, "the prevention of this lesion is very much under the control of the accoucheur; and I cannot but consider that, with ordinary care, by keeping the bladder empty, and still more, by never allowing the head to remain long in its passage through the os externum, this serious injury would not so often occur. I am aware that in thus advocating the early delivery of the head, I am opposed to many of the most eminent obstetric writers. Still, when I reflect on the very many cases which have come under my notice, and find that in almost every case this accident has occurred after protracted delivery, I am strengthened in my own opinion."

The distressing and repulsive nature of the consequences of this accident, rendering the innocent sufferer a burden to herself, and an object of disgust to others, though she may be, at the same time, in all other respects, healthy, and capable of enjoying the blessings and comforts of this life; perhaps, too, of a constitution that may be tenacious of life, supporting her for a considerable length of time, in spite of the mental agony and

bodily discomfort she is doomed to endure; these considerations, we repeat, ought always to be present to the mind of an accoucheur, leading him to the minutest observance of every preventive means which careful attention to the subject can suggest, in order to avert so direful a calamity. And we unhesitatingly believe that no circumstance is more to be dreaded as a cause of the disaster, than an unusually long-continued pressure upon the soft parts by a firmly-impacted child's head.

A *Recto-vaginal Fistula* is a somewhat more manageable disease than the preceding, and our author introduces three successful cases, two of which he treated with the actual cantery, and the other by passing a piece of lint through the opening, and allowing it to remain until it caused inflammation sufficient to produce a healthy granulating surface. In the after treatment of all these operations, great stress is laid upon keeping the bowels confined by the frequent administration of opium.

The chapter on Polypus of the Uterus is introduced principally for the sake of advocating a modification of the usual form of operation—namely, that of removing the polypus, either by a pair of curved scissors or a blunt-pointed bistoury, immediately after the ligature has been applied. The object of this is to prevent the absorption of secretion from the putrid mass, and the consequences of such absorption. It is obvious, however, that even here some portion of putrid substance must remain in contact with that still possessed of vitality, until the ligature has come away. And when we bear in mind that Gooch's beautifully simple canula can be applied without any forcible alteration in the position of the uterus, or dragging down of its interior to bring the pedicle within the reach of instruments, (we here refer more particularly to those polypi which are situated within the cavity of the uterus, and attached near the fundus), there is probably no safer or more effectual operation for the removal of such tumours than the one recommended by Gooch, especially if the ligature be composed of what is called "gold twist," which will cut through a pedicle of even large size in a very short period of time. If the tumour be removed from the vagina as soon as the ligature is away, and the vagina carefully washed out by means of a syringe, as often as required, the chance of the operation being followed by purulent absorption and phlebitis, is probably less than that of evil consequences arising from the disturbance to the uterus necessarily occasioned by seizing the polypus with a pair of vulsellum forceps, and bringing it down within the reach of manipulation such as our author describes.

In his observations on Imperforate Hymen, Mr. Brown is of opinion, that the entire removal of the hymen, by a circular incision at the point of its junction with the labia, is the best way of operating; and suggests, that when the usual crucial or stellate incision is performed, vaginitis may be set up by the friction of the divided portions upon each other, produced by every movement of the body. It is difficult to determine why such an apparently simple operation as division of the hymen should be so frequently attended with serious and sometimes fatal peritoneal inflammation. Langenbec attributes the tendency to inflammation to long retention of the menses; and, with reference to a case reported in a recent publication,* the writer of this article raised the question, whether

* *Cases in Midwifery.* By J. G. Crosse, M.D., &c., p. 141.

In such cases it might not be advisable to make a simple puncture, to evacuate slowly the retained fluid, and leave the patient to recover entirely from the effect of retention of the menses, before any other means are taken to open fully the passage into the vagina?

We have already carried our observations to such a length, that we must now refer our readers to the work itself for further information of its contents; and we can do this with the more satisfaction, because we believe it to be well worthy of attentive perusal. The concluding chapter, on Ovarian Dropsy, though containing but little of novelty, presents an exceedingly good general view of what is known upon the subject up to the present period. And in closing our remarks, we feel bound to state, that whatever may be the intrinsic value of our author's opinions and practice, both of which are entitled to much consideration, we can scarcely praise too highly the extreme care and attention he devotes to every minute circumstance that appears likely to promote success in his operations. If operations of a delicate nature, performed upon parts easily susceptible of injury, are to be successful, the patient must not only be prepared beforehand, and judiciously managed afterwards, but all unnecessary waste of time during the operation, all needless manipulation, all unneeded-for exposure, must be scrupulously avoided; and in all these points, before, during, and after operations, Mr. Brown appears to exercise that diligence, carefulness, and perseverance so essential to aid the surgeon in bringing his cases to a prosperous and satisfactory termination.

E. Copeman.

REVIEW III.

Handbuch der Speciellen Pathologie und Therapie. Redigirt von RUD.

VIRCHOW. Band 1, Heft 1.—Erlangen, 1853.

Manual of Special Pathology and Therapeutics. By R. VIRCHOW. (The Chapters on Fever and Inflammation.)

SINCE the earliest periods in the history of Medicine, the two most constant and singularly remarkable morbid processes manifested by the human body—those which have most strongly arrested attention, which have most invited observation, and which, as inquiries became multiplied, and the elements of Medicine as a science were gradually brought together out of the chaos of isolated facts, gave rise to the most profound theories and abstruse speculations of its various schools and professors—are, without question, the groups of diseased phenomena which, to the lasting detriment of science and the long-continued impediment to the advance of true knowledge, were early designated by the metaphorical names—Fever and Inflammation. Contrary to what might be at first supposed, the human mind, in many of its earlier efforts in various branches of inquiry, would appear to have attempted to grapple rather with the abstract than the concrete; to have overlooked, or, at least, to have given but little attention to the immediate, the positive, and the tangible, which it valued and used chiefly as furnishing the basis for speculative opinions, imaginative and often fanciful and extravagant theories. In this speculative process, the ideas formed about one subject of inquiry were eagerly grasped at to illustrate or explain those

of another, when any parallelism of phenomena or resemblance in conditions, however non-essential in either or both, could be traced. To this source is undoubtedly to be attributed the vast number of metaphorical and abstract terms so common in all languages, and which so particularly abound in the nomenclature of medicine. Thus, one set of phenomena, about whose nature, connexion, and causation, no definite knowledge existed, was freely used to illustrate, explain, and, in many instances, to give names to another set, till a metaphorical verbiage came extensively to supply the place of positive ideas; while ignorance and error lay concealed beneath a false, unreal, and superficial science that dealt only in words. But let us not take credit to ourselves for greater wisdom in our generation than we are entitled to. To this day the consequences of this false system of acquiring knowledge remain. It cannot be denied that the same erroneous direction of mind still prevails, if not generally yet only too extensively, and perhaps more so in medicine than in any other department of human knowledge.

The constancy of their phenomena, and the frequency, extent, and great influence of these two diseased processes—Fever and Inflammation—on human life, soon engaged attention. One phenomenon they possessed in common—that of increased temperature; the most obvious parallel for this was to be found in the heat produced by fire, and in this resemblance originated the names which they have since retained in all languages. In many respects the two processes presented great and striking points of resemblance to each other; it must likewise have been soon found that they often co-existed; and it is only to be wondered at that they so rarely came to be recognised as distinct. In the combination of both, a kind of debatable territory was found; and in the term “inflammatory fever,” still retained, may be yet seen the immense difficulties, if not impossibility, of assigning absolute limits to each.

In all times, these two great morbid processes have engaged the ablest minds ever devoted to the science of medicine; theory, observation, and experiment have been enlisted together to elucidate their phenomena, and each new addition to the various agents of scientific medical research has been brought to bear on their investigation. How far we have advanced in this inquiry, how far our knowledge of these great diseases may be now regarded as definite, complete, and satisfactory, and answering the requirements of our science, after the combined labours of modern physiologists, pathologists, and clinical investigators have been devoted to them; after the brilliant results which have been obtained by chemistry and histology, we shall place before our readers in a critical examination of the pages now about to be brought under consideration, which emanate from one of the master-minds of modern medicine, himself an original inquirer of the highest order, and one than whom, perhaps, no other living is so well qualified to appreciate and utilize the contributions from all branches of medical science, which pour in to elucidate the pathology of fever and inflammation.

Of all the various phenomena presented by fever, that of the elevation of the temperature of the body, in whole or part, is, perhaps, the most constant and remarkable, and has also attracted most inquiry. With Galen, the *calor præter naturam* was an essential phenomenon; many have, however, refused to recognise it as either constant or essential. While its

determination rested on the fallible evidence of sensation and the application of the hand, it was obviously impossible that the question could receive any very definite solution. De Haen was the first to employ the thermometer to ascertain the temperature in fever, and his observations went to show that, even in the rigors, there was an increase of temperature, in some cases even so much as 2, 3, and even 4° R.; and that the slightest febrile conditions are attended with an increase of heat, which is, also, sometimes the only observable phenomenon. Gierse, Roger, Traube, Zimmerman, Baresprung, and others, have repeated and confirmed these experiments: so that Virchow remarks, it is no longer doubtful that, in the *calor præter naturam*, as stated by Galen, is the substance of fevers. The rigor is but a peripheral phenomenon, and while the outer parts are frozen, the inner burn. The blood is to be regarded as the seat of the febrile temperature, and the more blood a part contains, the warmer it becomes, and thus the term "fever" may be regarded as not altogether figurative, febrile or pyretic diseases being those in which an increased combustion takes place. We shall not stop here to examine the various theories which, from age to age, were adopted to explain the phenomenon of the increase of temperature in fever. Certain physical conditions, such as increased velocity and friction of the blood particles, cannot be altogether excluded from consideration; but it must, in the present state of science, be admitted, that the chief source of increased temperature in the human body is to be sought in the chemical development of heat attendant on the nutritive process; and to the increased consumption of organic material is to be attributed any increase of temperature. But in fever, it is not alone the materials supplied by nutrition that are subject to combustion, but also actual constituent elements of the body; not only the fluids, such as blood, parenchymatous fluids, oil, and fat, but even the solids, as muscles, glands, and bones.

The increased consumption of material has also been assigned to an increased respiratory action as its cause, the exhaled carbonic acid being taken as the standard of measurement; and the few exact observations which have been made, appear to show that there is an actual increase in the process of oxidation. This, however, is but an imperfect mode of estimating the amount of oxidation, for many organic substances become oxidized without the production of carbonic acid; on the other hand, Moleschott has well remarked, that the origin of heat in the body is not to be solely attributed to the combination of oxygen with organic materials, but that in the separation of bodies which become only in part oxidized, is to be found a rich source for the production of animal heat.

If, with Virchow, we regard heat as the pathognomonic symptom of fever, and consider it to be dependent on an increased consumption of the constituents of the body, we have yet to seek the cause of this increased action in the system. This cause, the *causa proxima* of fever, Virchow regards as internal, and connected with the body itself, in contradistinction to the external causes acting on the body from without. Various changes in the constitution of the blood, productive of a *materies æris*, fever-stuff, the pyretogen of Eisenmann, have been assumed as the cause of fever; and while we may hold that, in many cases, some such change is actually produced, we are not yet in a position either to determine its precise nature or to understand how it is itself brought about. Some facts, determined by recent

experiment, would appear to show that the proximate cause is not to be sought in the blood alone, and to point to deranged conditions of the nervous apparatus as more likely to furnish us with a solution of this question.

There is no reason for seeking other sources for the fever heat than those which furnish the natural heat; only it may be considered, that in fever, the usual processes are in excess, and this excess may be traced to either of two sources. We may suppose that certain nerves preside over the development of heat, or that its production is moderated by certain nerves; in the former case the febrile heat is to be attributed to an increased, in the latter to a diminished innervation. The experiments of Becquerel and Breschet, and Helmholtz, show that nervous excitation can produce muscular movement, and that this is attended by increase of temperature. In opposition to this, Bernard has proved, that section of the sympathetic in the neck is followed by rapid increase of temperature in the corresponding half of the head. Taking into account the various phenomena which attend the onset of a fever, characterized, as it is, by weakness, prostration, and loss of energy, especially in the muscles and sensitive parts, Virchow concludes that the elevation of temperature, which is found to be amongst the earliest and most constant of the symptoms, is to be regarded as a paralytic phenomenon, produced by loss of power in the nerves, which constitute the natural moderators of the development of heat. With regard to this moderator-function but little is known, and it seems to be equally assignable to opposite portions of the nervous system; there appears, however, to be considerable reason for selecting the *vagus* as one of the most important agents in the production of the febrile phenomena. Besides the elevated temperature, the alterations in the circulation, and the movements of the heart, the lesions of digestion, anorexia, vomiting, &c., as well remarked by Virchow, fall within the domain of the physiological influence of the *vagus*. E. H. Weber has shown that irritation of the *vagi* causes an arrest of the heart's action; while it is long known that, after section of these nerves, a remarkable acceleration of the pulse takes place. The lesions of the heart, lungs, and stomach thus fall into one category, and, as observed by Traube, a large number of the symptoms of fever may be referred to a weakened, more or less paralytic, condition of the *vagus*. This observer has found that *digitalis* acts as a stimulant on the regulator nerve of the heart, and concludes that the diminution of temperature which he has noticed after its employment is produced by the diminished velocity of the blood-stream. Ludwig and Hoffa have ascertained that, by moderate irritation of the *vagi*, the lateral pressure of the blood in the arteries is lessened; while Volkmann and Fowelin have shown that it is increased after section of these nerves. These very remarkable physiological experiments demand especial consideration, as they not only much modify our previous views of nervous action, but supply new and important therapeutic indications. It is to be observed, however, in connexion with the experiments just detailed, that those of Lichtenfels and Frolich appear to show that, in the normal condition, the frequency of the pulse and the temperature are independent of each other; Virchow, therefore, concludes that the fever temperature is an essentially compound phenomenon, dependent on nervous, physical, and also nutritive conditions. In estimating the conditions of the circu-

lation, some points here noted are worthy of consideration; the heart's contractions will be found an imperfect criterion of the state of the circulation in many instances, as rapidity of its beats is often found to be attended by an incompleteness and weakness of its action; and so imperfect may the single contractions be, that the mass of the blood, instead of moving with increased velocity, is retarded, and flows more slowly. The state of the circulation is, therefore, to be estimated not so much by the energy and number of the heart's contractions, as by the general tension of the whole vascular system. The question of the tone of the vessels, and its alteration in fever, has been often already considered, and a large part assigned to the vaso-motor nerves in the production of such changes. In regard to the phenomena under consideration, the cerebro-spinal and the sympathetic nerves appear to present somewhat different properties: thus, in paralysis of the former, there is always a diminution of temperature in the parts supplied by them; while the sympathetic nerves retain their power even in cases of complete paralysis of the cerebro-spinal system, and this power may be increased when the branches connecting them with the spinal marrow are paralyzed, and this may likewise be accompanied with an increase of temperature. If any centre for this regulating nervous force is to be sought, it would appear, most probably, to reside in the medulla oblongata, near the roots of the vagi.

Fever, then, in the opinion of Virchow, essentially consists in an increase of temperature, which is caused by an increased consumption of material, and appears to have its origin in changes in the nervous system; these changes may be considered to affect the regulating or moderating function of the nerves, and thus to be the cause of the increased consumption of material. He believes that every given disease and every lesion may pass into a fever if it invades the regulating centres of the consumption of nutritive material. If these centres are to be sought in the nervous system, an abnormal condition of tension must be admitted, which is called into play by the causes exciting the fever, and which finds no solution in the natural processes. As the force of the moderating centres is arrested, the consumption of material advances, and, in proportion, the heat of the body increases, and the particular point of the commencement of fever is reached. At first we see only the weakening of the corporeal and mental powers which follows directly on the condition of tension.

On the subjects of crisis and critical days, medical opinion has undergone much change in modern times; many have gone so far as to reject altogether the belief in critical days, and to regard the crisis, when it occurred; rather as the result than the cause of the cure. Recent observations, especially those with the thermometer, show that the excretions in the course of febrile diseases have very various significations; that they may be regarded as co-effects of the lesion, being sometimes without influence on its termination; sometimes accompanied by exacerbation, when they may be called symptomatic; and sometimes appearing to be the means of resolution, when they may be called critical; and, lastly, they may occur subsequent to the period of resolution, and as an enforcement of it, when they will be designated post-critical. Thermometric measurements would appear, however, to lend considerable support to the doctrine of critical days.

Such is a brief outline of the views which now appear most tenable with

regard to the nature of fever; much is yet left undefined, and there is, also, much which will present itself to the mind of every practical physician as difficult to be brought within these limits. To those especially who have pursued close and accurate clinical studies of typhus fever, many points in its pathology will, doubtless, appear to be incapable of solution by the views of Virchow.

Of the indications for the treatment of fever dwelt on by Virchow, we shall here notice only those directed to the nervous system. These embrace the use of the so-called nerve medicines, to which belong digitalis, quinine, arsenic, and a number of other substances, chiefly from the group of vegetable alkaloids and the metals. The action of many of these medicines in lowering the temperature in fever has been fully established. As already noticed, Traube has shown it to be probable that digitalis acts on the medulla oblongata and the roots of the vagi. Duméril, Demarquay, and Lecoq have sought to prove that quinine acts directly and powerfully on the sympathetic system, as already suspected by Lobstein. These results are yet but small; still, in our minds, they foreshadow the approach of a rational and scientific system of therapeutics for fever.

INFLAMMATION constitutes in some respects the analogue, in others the opposite, of Fever; next to fever it must be considered the most remarkable of all pathological processes. Virchow describes it as one of the general forms of phenomena under which a number of most different local diseases may manifest themselves. As in fever, the most remarkable phenomenon of inflammation is an elevation of temperature, which, in the former was considered to be general, while in the latter it is only local. We shall omit here all notice of the various doctrines held as to the essential characters of inflammation, from the symptomatic quatrain of Celsus to the fluxus, stasis, constriction of the capillaries, obstruction of the blood, and *error loci* of the globules. These points will be found discussed at length in the original. Two most opposite conditions of the bloodvessels, spasmodic action, and paralysis of their walls, have been assigned as the cause of some of the most essential phenomena of inflammation, and each has received the support of able investigators. It is now, however, pretty generally agreed on, at all sides, that the local alterations of the circulation, in themselves neither simple nor uniform, are insufficient to define inflammation; it may be stated that not any one of the many phenomena assumed by various observers to be essential, is constant, and that even opposite states of the blood and bloodvessels may present themselves in succession within a short period in the same inflamed spot. The experiments of H. Weber, in particular, show that the paralytic as well as the spasmodic states of the vascular walls are of secondary importance in relation to the stasis; and, on the other hand, lend much weight to the so-called attractive theory. This theory is based on certain physical relations found to exist between the blood, the vascular walls, and the parenchyma. In estimating this attractive force, we have to take into account many necessary physical conditions of the circulating fluid itself, as to viscosity, cohesion of its particles, and the molecular attraction exercised between them and the walls of the vessels. In accordance with these principles, it may be stated in a general way that the more the blood adheres together, and the less it is attracted to the vascular walls,

the more easily will it pass the capillaries; but the more its cohesion becomes diminished, and the greater the molecular attraction between its particles and the surrounding walls, the more readily will it stagnate. This, then, is the basis of the attractive theory. Thus an inflammatory irritant may attack a certain tissue, produce in it chemical or physical changes, and in this way induce an altered molecular attraction of the blood, an endosmotic or exosmotic current, or a change in its internal cohesion, and therewith a scattering of the corpuscles throughout the stream, an attraction of them to the walls of the vessels, and thus finally an obstruction to the current. Thus considered, Virchow accepts the attractive theory as capable of explaining the stasis, but not therefore the inflammation.

The phenomena of stasis have been frequently and accurately observed, and as well described, so that we need not here repeat the details of the diminished velocity, partial intermission and oscillation of the stream, gradual disappearance of the intercellular fluid, relative increase of the blood-corpuscles, and their lessened diameter, till they fill the entire vessel, and the whole mass stands still. Coagulation of the fibrine has been asserted to be one of the first and most necessary steps (Cruveilhier); but so far is this from being the case, that Paget has found the blood still fluid after three days' stasis. The most constant phenomenon of stasis is the thickening of the blood, and this is induced by the loss of the watery constituents of the plasma.

But the essence of inflammation is not to be found in the local lesions of the circulation, and we must therefore take into consideration the changes in the surrounding tissues, which also participate in the inflammatory action. The most important question connected herewith is that of the seat and nature of the *exudation*, about which much misconception has prevailed. Virchow has shown that the exudation is to be regarded only as the nutritive fluid which has passed from the blood into the elements of the tissues, and is not to be separated from them or isolated—which has in fact become part of these elements, being received within their cells or basic substance, and which can hardly be regarded as distinct from them. The exudation must not be regarded as the proper product of the inflammation, which is rather constituted by the materials resulting from the destruction of the tissues, though it cannot be denied that the exudation itself may undergo changes.

The origin of the exudation is traceable to two sources. It may be regarded as the expressed blood-fluid resulting from the mechanical pressure under which the blood moves in the finer vessels; or it may be considered as the result of an increased attraction of the blood-elements by the tissues, an increased diffusion of material, in fine, as a nutritive educt. These views require some modification in regard to the free superficial exudations, which have some similarity to secretions. Virchow gives this subject a special examination, in which we regret we cannot follow him.

In the opinion of our author, then, the inflammatory process in its proper essence is to be regarded as a local lesion of nutrition (a view, as it appears to us, long shared by others), and, like all local lesions of nutrition, depends on the interchange of materials between the blood and tissues. —

It yet, however, remains to be inquired, how this local lesion of nutrition differs from others, as all such lesions manifestly do not constitute inflammations. The local elevation of temperature has been long regarded as the criterion and proper clinical expression of the inflammatory process, and, as is well known, formed one of its four cardinal phenomena in the symptomatology of Celsus. Experiment and observation give somewhat conflicting results as to the exact conditions of temperature in inflamed parts; the weight of argument, however, appears to bear on the side of an absolute increase; this has been determined in one case by Hunter, and more recently by the thermo-electric experiments of Becquerel and Breschet. Virchow considers it highly probable that heat is actually produced in inflamed parts, but that the temperature of the foci of inflammation is to be regarded as the expression of two distinct sources of heat, one of which is to be sought in the blood, and the other in the parts themselves. The local elevation of temperature produced by the influx of blood, especially of heated febrile blood, must on its side contribute essentially to the increase of the local metamorphosis of tissue. The nutritive processes must certainly be influenced by an elevation of 2° or 3° R. in the temperature of the parts in which they are taking place, and new chemical relations must be formed.

Looking to the assemblage of phenomena which are combined in the inflammatory process, it will be seen, says Virchow, that they exhibit a very close relation to those of irritation; and that between inflammation and irritation only a quantitative difference is to be found, the former following step for step on the latter and their phenomena being distinguished only by the characters of relative greatness and extent. The phenomena of irritation, though also dependent on material changes of the parts, have usually more a functional dynamic character; those of inflammation depend more on nutritive lesions. As long as the lesion continues functional, we speak of irritation; when, however, it extends to nutrition we call it inflammation.

In the rapidity and excessive force with which the nutritive changes are wrought, will be found a characteristic of the inflammatory process which distinguishes it from ordinary lesions of nutrition, and gives it the appearance of an increased combustion of material; to this may likewise be added the increase of temperature of the exudation, which has been shown to be essentially but an increase of the natural nutritive fluid passing out from the blood to the tissues, and becoming incorporated with their elements: only a portion of it is, therefore, to be ascribed to the inflammatory action as its origin.

Another important question in connexion with the phenomena of inflammation, is that of hyperinose (Franz Simon), or, as it is more intelligibly named, increase of fibrine. It is unnecessary here to pass in review the various opinions with regard to slowly coagulating and hyper-oxidized fibrine, the oxyprotein and bradyfibrin of authors. Various kinds of fibrine have been assumed to exist, and also various modes of disease in fibrine. Virchow has long been an antagonist of these views as to chemical differences in this substance, all the observed differences in the mode of coagulation being referrible to mechanical conditions, such as the viscosity of the blood-fluids, and the relative quantity of the

fibrine. This substance is usually submitted to examination in its coagulated state, and much of the difficulty of exactly determining its nature depends on this circumstance. Virchow proposes to call the substance, which just as it appears to view becomes coagulable by the action of oxygen, fibrinogen. We cannot see that any particular advantage is likely to attend the introduction of a new name, unaccompanied by any additions to our knowledge of the substance thus designated. However we may regard the question of the existence of specific forms of fibrine, it must be admitted that an increase in the bodies belonging to the fibrine group, very generally though not constantly attends inflammatory action, though not as a specific and peculiar process, for the same is found to be the case after abstraction of blood.

There appears, then, to be no specific ontological character for inflammation, either in the conditions of the circulation, the state of the blood, the phenomena of pain, heat, tumor, or redness; neither has it any specific difference from other lesions of nutrition, but is characterized by its extent, by the rapidity of its course, and especially by the character of the lesions it produces; its destructive tendency is that by which it is most remarkably distinguished from the simpler lesions of nutrition, and here especially the pathological differs from the physiological process.

It will be seen that inflammation constitutes a very complex process, the essence of which it is excessively difficult, if not impossible, to determine and give expression to. It is also much modified in the phenomena by which it manifests itself according to the dignity and peculiar properties of the parts in which it is developed, and thus will sometimes possess one, sometimes another dominant character. Much of this difference of its phenomena is likewise to be attributed to the nature of the irritant which calls the inflammatory process into action, and also much to the pathological predispositions of the part affected. These predispositions embrace those minor forms of lesion which are consistent with the condition of health, but yet induce a certain weakness and a greater capability for becoming the seat of more extensive and important changes, and moreover form an obstacle, often considerable, to resolution.

With these local predispositions may likewise be grouped those of general influence, and which have a specific dyscrasic character, such as the scrofulous, syphilitic, gouty, as well as the rheumatic and septic; and though we may not in the present day admit a special *materia peccans*, syphilitic, gouty, or scrofulous, as existing in the blood, we have yet reason to believe that in many cases foreign heterologous substances are developed, in the seat of inflammation, which give to the further course of the disease a special character. According as these changes result in the elimination of the foreign substances from the blood, or on the other hand induce an impure condition of this fluid, may the inflammation be regarded as benign, depurative, or critical in its character; or on the contrary, it may possess an unfavourable or malignant influence. Another group of inflammations is, however, to be recognised, in which, from the direct introduction of a specific irritant from without, the character of the inflammation is altered from the first, as we see in the whole range of toxic inflammations.

• The sthenic inflammation is to be regarded as the pure form of the

process when it takes place under favourable conditions. The asthenic may be regarded as pure inflammations occurring in weakened parts or bodies. Another form may be recognised, the hypersthenic, whereby is understood that form of inflammatory lesion of nutrition in which, either from a particular predisposition or a peculiar kind of irritant, there is such a development of force, or such a remarkable metamorphosis of tissue, that the inflamed part is entirely destroyed, or that a constant development of new inflammatory products takes place; to this group Virchow refers many of the irritable inflammations of English authors.

The results of inflammation and its sequelæ are deferred for subsequent consideration, and the remaining pages of this section are devoted to the subject of treatment, wherein we find nothing to engage our attention at present.

We are conscious that we have in the preceding pages given but an imperfect view of the opinions of this eminent pathologist on these, perhaps, the two most important subjects in the domain of medical science. We trust, however, that we have succeeded in giving our readers a just idea of the broad and truly philosophic manner in which he treats them. We are far from saying that the last hand has been put to the picture in either case; but when we consider the immense difficulties of the subjects, their rank in pathological science, and above all, the way in which the determination of their least essential elements is dependent on the state of collateral departments of our science, it is not to be wondered at that our knowledge is not more precise and definite. That the concentrated lights now thrown on them have in many instances but shown us new difficulties and new paths to be explored, must be admitted on all hands; yet, if we have not gained a complete or perfect knowledge of what fever and inflammation are, we may safely say that we have found the right road, and that the goal lies before us, remote though it may be; and the student who takes the essays of Virchow for his guide, will at least be saved from error, for in his large philosophic views erroneous doctrine and dogmatic fallacies find no place.

—Robert D. Lyons.

REVIEW IV.

Traité Clinique et Pratique des Maladies des Enfants. Par MM. RILLIET et BARTHEZ, anciens internes lauréats de l'Hôpital des Enfants Malades de Paris, &c. Ouvrage couronné par l'Académie des Sciences et par l'Académie de Médecine. Deuxième Edition, entièrement refondue et considérablement augmentée.—Paris, 1853. 8vo. Tome premier, pp. 844. Tome deuxième, pp. 748.

A Clinical and Practical Treatise on the Diseases of Children. By MM. RILLIET and BARTHEZ. Second Edition.—Paris, 1853. First and Second Volumes.

THAT student is young indeed in the department of pædiatrics who requires to be informed that the authors of the 'Clinical and Practical Treatise' have earned therein a position not surpassed by that of any pathologist of the present day. As systematic writers, sound teachers, and able practitioners, we believe this judgment to be fairly their due,

though the reputations of Trousseau, Barrier, Valleix, Mauthner, and others, be not at the same time forgotten. Ten years ago, MM. Rilliet and Barthez laid before the profession the result of their united labours in the form of three goodly volumes, numbering in the total about 2400 pages. Now they present us with a new and greatly improved edition of their work, of which two portions are before us, and the remaining third we hourly expect.* It is pleasant, amidst much professional selfishness and jealousy, thus to witness two eminent masters in their *spécialité* continuing their conjoint exertions as labours of love both to their common science and reciprocally to themselves.

"Separated by distance, but ever united by community of thought, we have continued to labour with perfect identity of view towards the perfecting of our work." (p. 3.)

The *one* remaining, in the arena of his earlier toils, actively connected with the scientific movement animating the medicine of the capital of France; the *other* called to exercise the practice of his art in a distant city, and led to follow the important teachings left as an heritage to the faculty of Geneva by the Odiers and Jurines. We repeat, and with all indifference to any charge of hypersentimentalism, that, constantly reminded as we are of the "poverties of the spirit" (to use no stronger terms) entering into the ties of our professional and social connexion, we know not whether it is more a source of pleasure to us thus to witness the almost paternal union of these *ancient intervals*, or of profit in recalling how it should be equally our own disposition: "*σπονδαζόντες τηρεῖν τὴν ἐνότητά του τριφυλλοῦ ἐν τῷ συντάγματι τῆς ἐργασίας.*"

From the agreeable associations connected with MM. Rilliet and Barthez, the high position they occupy as authorities on the pathology, &c., of early life, and from the great value of much now superadded to, or interwoven with, the text of their former edition, it might be supposed that we intended to enter upon a critical or analytic exposition of the chief novelties presented by the volumes before us. But this is not the case and chiefly for the following reasons: very many, if not nearly all, of the later investigations and more matured opinions of the authors in relation to individual diseases, have already appeared, in the form of papers (of great value), in the journals of the day.† These monographic essays have ere this produced an influence upon the branch of medicine to which they related—an influence, however, not the less valuable because excited previous to the systematic union and publication of their contents in the new edition of the great and laborious undertaking now upon our table. This influence, it is hoped, must have been also felt by the readers of this journal, in which due account has been taken, either by express statement of the progress of our authors, or by the more silent weight their particular investigations have exerted in modifying our own opinions and judgments. Further, we would remark that the very voluminous character of the 'Treatise' would forbid, under any circumstances, a detailed analysis of its emendations; and, moreover, it is not our practice in general to do more with second editions than to treat them very cursorily, pronouncing on them simply an *ex cathedra* judgment relative to the

* The concluding volume has since appeared.

† Archives Générales de Médecine. Gazette Médicale.

more important changes the original work has undergone. Forbidden, then, as we feel ourselves, to dwell upon the histories of individual diseases, yet we cannot pass over the important treatise of MM. Rilliet and Barthez with but a simple record of its new appearance. Out of compliment to the great reputation of the authors, and from the importance of some circumstances modifying the general pathologic doctrines of the present volumes, we purpose selecting a few topics worthy of some consideration, and as forming a fitting propaedeutic to the study of the details of the 'Treatise' by those who, having entered upon the study of medicine within the last decade, have to form their acquaintanceship with MM. Rilliet and Barthez through the medium of the new edition.

The first circumstance we would refer to as having enlarged and modified the experience of our authors, is their having, since their first publication, enjoyed the advantages afforded by private practice. The results thus gained we find now added to those derived from their close and extended hospital instruction. A professional education, limited in its clinical departments chiefly to either public or private duties, cannot fail of presenting disease under a partial aspect, though if the treatment of it is afterwards to be prosecuted under the particular phase in which its pathology has been studied, no great detriment may ensue either to patient or practitioner. But without derogating from the knowledge and abilities of those whose study and practice have been necessarily thus circumscribed, we may safely maintain, that to such as are desirous of prosecuting the study of medicine on a broad and comprehensive scale, of viewing it in all its relations and dependencies, and, moreover, of becoming, like our authors teachers of its pathology and practice, then the results of civil and hospital, public and private, practice must be duly joined together. This necessity bears with particular force upon the branch we are discussing, in which the influences of hospital hygiene are seen to be exerted in a peculiar and forcible manner, on the one hand giving rise to several affections not seen at all in private, and on the other importantly modifying those which are common to the patients of both classes. It is in the French capital, perhaps, that the greater contrast is observed between the two, for there the large and crowded hospitals for sick children and foundlings allow full scope for the operation of the deleterious influences springing from the consolidation of numbers within limited space, and from the artificial feeding and unnatural kind of life led by their little inmates. From these and analogous circumstances, erysipelas, peritonitis, gangrene of the lung, diphtheritic forms of croup, &c., inflammation of Peyer's glands, muguet, malignant coryza, and sclerema, are seen in such force and variety, that no extent of private practice amongst the paying classes of society could ever exhibit. As MM. Rilliet and Barthez remark:

"Under the influences of an abominable hygiene, we find that interminable series of grave disorders originate and spread which we far more rarely witness in private practice. Some of these affections are evidently the result of a prolonged stay in over-crowded wards. We had aforetime coined a word to express them which we may be permitted here to recall, notwithstanding its etymology be not strictly grammatical, viz., that *d'hôpitalité*." (p. 7.)

"The prognosis of the diseases of new-born children, in particular, deduced from observations made in the hospital for foundlings, is of perfect exactitude, so

far as it relates to the patients treated in this institution. But we cannot generalize upon such facts as these without forgetting those judicious words of Baglivi: *sub sole romani scripsi*. The more we advance from early infancy, the closer do the diseases of the hospital approach those of private life; but still it is necessary to draw a very strict demarcation between primitive and secondary disorders, to enable the facts gathered in the establishments appropriated to the treatment of the diseases of second childhood to be made available in practice." (p. 30.)

The public practice in a great town like London, amongst the children of the lower classes living near the banks of a muddy, filthy river, in localities, too, where the exigencies of space and of high rent oblige the parents with their children to inhabit a single small room, we believe to approximate most nearly to the practice of the hospitals of the Continent. The miasmatic atmosphere there generated is here witnessed, and yearly the same reunion of anti-hygienic circumstances creates the like *veritable climat pathologique* dwelt upon by our authors (p. 7). To these facts our own experience will bear witness; and we may refer also, in illustration of them, to the remarks of Dr. West, in his 'Lectures' on croup and diphtherite, diarrhoea and dysentery.* But if, on the one hand, the hospital and public practitioner may thus witness particular forms of disease on a large scale and in severe intensity, be able likewise to avail himself of continuous necroscopic investigation, and possess, to a great extent, a control over the carrying out of measures which the friends of private patients would often object to; the advantages of *each* practice, on the other hand, are not the less evident.

"The family attendant witnesses the child's birth and development; he is acquainted with its hereditary tendencies, can follow its progress in life, and by its past course judge of its future. Called in most frequently at the outbreak of the malady, he himself observes the fugitive but important symptoms characterizing the first period of the disorder, a careful mother watches the punctual execution of his orders, and places him in possession of a complete knowledge of all that has occurred in the intervals of his visits."

"A great part of these advantages is denied to the hospital physician." (p. 4.)

We took occasion in a former article (vol. xi. p. 126) to express our belief that a refined analysis must lead to the doctrine that all diseases (with the exception of certain traumatic ones) have their genesis in some change or modification of *power*. In other words, that they are primarily *functional* in character, however rapidly visible structural alterations might show themselves during the course of any one of them. Side by side with our own opinions stood those of another writer,† in which a like doctrine was affirmed in its particular relations to zymotic diseases, it being shown that in the latter the influence of the ferment is not material, but *dynamic*; not consisting in the introduction or substitution of components, but in the *propagation of force*. We now lay emphasis on the fact that, whilst it is to be fully and fairly admitted, that structural changes or anatomic lesions ensue very early, or even *appear* to coexist from the first, in many diseases, apparently giving rise also to the more prominent of the symptoms, and sufficiently explaining the cause of death:

* Lectures on the Diseases of Infancy and Childhood, second edition, pp. 231—253: 229—241. See also some observations by M. Rilliet in vol. xviii. of the *Journal für Kinderkrankheiten*, p. 378.

† British and Foreign Medico-Chirurgical Review, vol. xi. p. 163.

yet it cannot be denied that in many other instances such changes and lesions as may arise do not explain the symptoms, and are equally deficient in rendering a satisfactory account of the "why and because" of the fatal event. Whilst the records of morbid anatomy must ever form a most important part in explaining the nature of a vast series of diseased actions from which the open manifestations or symptoms spring, and in often sufficiently accounting for the arrestation of all vitality, yet it is clear that the archives of the *mortuarium* will for ever fall short in elucidating what is most essential, and as yet hidden in darkness. Nor do we see that the present (in some cases exaggerated, as it appears to us) application of the microscope towards a minuter and more recondite analysis of structural changes invisible to the unaided eye, will lead us a step nearer to the essential nature of many affections, although it may establish the presence of material changes, of whose existence before we were entirely ignorant. We say that we are really unable to perceive any magic power of solution brought into existence by this instrument over the *causa* and *ori* of many diseases hitherto, and as yet, unreachd by our analysis, and which still appear to stand within the limits of the knowable, though only to be solved, as we believe, by the use of a calculus of a totally different character. It has been well said by a great writer,* the force of whose saying a translation would not do justice to, that— "Was man wahrhaft verstehen will, von dessen *entstehen* muss man einen deutlichen Begriff haben." Here the object to which we should be striving appears to us not to reside in the lesional curiosities laid bare by the "anatomic objectives," but in the functional energies which give rise to them, or go before their production; and still more essentially (though here perhaps we may be even at fault) in the antecedent *causes*, a power which initiates these very functions. So often as medicine will persist in believing that the utmost step to which we can generalize must be the landing ourselves on some anatomic alteration, so often as it seeks a treatment alone based upon or directed to it, so often we believe it will miss its mark. This, however, is no new doctrine; but we venture to repeat it, though its spirit has been taught long ere this. It was affirmed by Lobstein,† repeated by Professor Alison,‡ and again forced upon our notice by Dr. P. M. Latham.§ Holding the opinions we have just expressed, it was with much satisfaction we came upon the following paragraphs in the preface to M. Rilliet and Barthez' work:

"The mode in which we treated our subject in the first edition of this treatise secured us the reproach of being too anatomical; now perhaps we shall be found fault with for having devoted too great a space to questions of doctrine and etiology. But we would reply, that on leaving the benches of the lecture room, we, along with a great number of our illustrious predecessors, have been brought to recognise the truth, that the autopsy is not the climax in medicine, and that the study of the dead body is not alone able to present us with the key to the phenomena of life."

"But we would wish not to be misunderstood, nor accused of professing for pathological anatomy a disclaim which is farthest from our thoughts. . . . But as years have run on, the horizon has become extended and the mind enlarged. We have asked ourselves whether, under the influence of pre-occupations too exclu-

* Goethe.

† *Traité d'Anatomie Pathologique*, liv. i. § 299.

‡ *Outlines of Pathology*, &c., p. 39.

§ *Lectures on Diseases of the Heart*, vol. i. p. 52.

sively anatomic, we have not often taken the effect for the cause, the result of the affection for the affection itself." (p. v.)

Along with this modification of their views, the authors have been brought *pari passu* to the fuller adoption than before of a humoral pathology; or, to use their own expression, "Without entirely quitting the path of solidism, we have made one step nearer humorism and vitalism." (p. v.) Humoral pathology has been so fully and lately discussed in the pages of this journal (vol. xi. p. 5 *et seq.*), particularly in its relations to fever, &c., that we need presume but very few remarks upon it here. With the gradual return to its doctrines made within the last twenty years by a very large proportion of the more eminent members of the schools of Europe, there has been a greater tendency to regard the diseased conditions of organs and parts as merely the local signs of more general disorder, or of some disturbance or contamination of a general system in the frame, the former being looked upon as merely the visible outbreak of the tumultuous spirit within. This tendency is acknowledged by MM. Rilliet and Barthez as holding an increasing growth in their own minds, and as having considerably influenced the opinions and judgments propounded in their present volumes. They observe that from the first they were always disposed to attach far more importance to the changes affecting the general health than to the topical disorder, but that now,

"Guided by the principle that Nature is niggard of causes but prodigal of results, we are convinced that the numerous local affections so sharply separated from each other, according to the present dominant ideas, are only the result of a small number of general morbid conditions." (p. vi.)

To discuss this matter in detail is by no means within the scope of our intentions; we shall simply refer to a few points connected with the pathology of children bearing upon it, or to such as a perusal of the pages before us have impressed upon our memory as worthy of comment, with the above statement of our authors in view. Before doing so we may just recollect to mind, that as the pathologic relationships of the blood become more developed, the more are we inclined to seek in its mutations of vital forces and competency, the genetic sources of a very considerable number of local diseases. The abnormal conditions which this fluid may present may be derivable from hereditary transmission or endowment, constituting one great variety of diathetic maladies; whilst endemic and epidemic sources of zymotic and other poisons give rise, from their contamination of the circulating mass, to another great class of constitutional affections. The subjection of the living organism, especially during its earlier years, to the various well-known anti-hygienic influences, by which the offices of nutrition and assimilation, &c., are seriously perverted, bring about the cachectic and anæmic states, and other general and fertile sources of numerous chronic forms, especially of local malady. It is probable that another source, or constitutional expression, of a certain class of affections, is to be looked for in some morbid general condition, of a dynamic character, of the nervous system, and totally irrespective of its occasional production by a more primary abnormal condition of the blood itself. In this greater or less general nervous affection, more commonly met with in women and children, the sensations and emotions become intensely felt, their reflex action on the system or special organs extreme,

the voluntary efforts of mind and sustained exertions of the voluntary muscles difficult, strange, and irregular, and the "*animus nec sponte varius et mutabilis*."* We are fully aware that we stop short, in comparison with some, in thus hesitating to resolve almost all the *neuroses* into a primary morbid condition of the blood, either of the character of cachexia, or as dependent on some special *materies morbi* or toxic element in it. But a careful consideration of several facts, such as have been well insisted on by Mr. Carter† especially, makes us pause before we surrender up *everything* to the blood. It may be admitted with Andral‡ that the 'impressibility of the nervous centres often becomes the more evident as the quantity of blood diminishes and the muscular system becomes weakened, and that in this state the least painful form of hyperæmia may give rise to the most serious functional disturbances of the cerebro-spinal axis; with Alison,§ that a morbid increase of the activity of the changes in any portion of nervous matter may readily become attended by an increased determination of blood, or other change in the circulation of the part; and with Dr. Laycock and others, that certain diathetic conditions of the blood are very favourable to the outbreak of the neurotic affection. Nevertheless, with these admissions, as also, of course, with that of the fact of there being a great number of the *neuroses* which undoubtedly are but the manifestations of a primary morbid state of the circulating fluid, whether of a cachectic or toxic character, we do not yet feel warranted in believing that *all* are so ||

In childhood, epidemic influences and infection will be found to be the sources of the greater number of its acute diseases. It is necessary to bear in mind, however, the marked differences which exist between those affections which are merely epidemic, and such as are both epidemic and infectious.

"The former may be seen in quite the young infant, as well as at puberty, as evidenced by the tracheo-bronchial and gastro-intestinal catarrhs; whilst the truly infectious diseases—those which we shall describe under the name of general acute specific maladies—are almost special to children who have exceeded their first or even second year. The eruptive or typhoid fevers, whooping-cough and mumps, serve as examples" (p. 8.)

The almost complete immunity of very young children from certain affections, chiefly of the zymotic class, has been explained by their comparative isolation or protection from the contiguity and sources of the particular morbid poison. The validity of this reasoning is to some extent admitted by MM. Rilliet and Barthez; but they maintain it does not explain the whole of the matter, and that it is necessary to suppose, that at the commencement of life the organism is not sufficiently prepared "for the hatching of the morbid germ that contagion deposits there."

"Is it not remarkable that the same observation applies to tuberculisation and some other hereditary diathetic maladies, which demand a certain development of organs before they can exhibit their open manifestations?" (p. 8.)

While the local anatomic changes and circumscribed lesions of organs

* Vide Alison, *op. cit.*, p. 710.

† On the Pathology and Treatment of Hysteria, p. 79, &c.

‡ *Precis*, tom. i. p. 18.

§ *Op. cit.*, p. 679.

|| See also On the Prevalent Treatment of Disease, by F. C. Skye, F.R.S.—London, 1853, p. 26.

are about the same (or analogous) in the child as in the adult, yet the cases are, perhaps, much more frequent in infancy than at any other age in which death happens; and yet no such change of structure, or even any evident alteration sufficient to account for the fatal event, can be found on cadaveric inspection.

"We see children succumb after having had profuse and continuous diarrhoea, or presented very serious nervous symptoms, while the intestines and the encephalo-rachidian system appear in a normal condition, or are too slightly diseased to explain the violence of the symptoms." (p. 18.)

As parallel with these cases, we may refer to others still more frequently met with, in which the disease runs through its course and terminates happily; and yet we shall have been quite at a loss to say which organ had suffered material lesion, or which particular group or series of symptoms were to be regarded as necessarily marking the true nature of the malady. On the other hand, we occasionally meet with, in children, such extreme and peculiar local alterations of structure and disease of organs, giving rise, during life, to great functional and local disturbances, and, finally, to death, which yet, during their whole course, were not resolved into their exact nature; nor has the local lesion (*a* lesion being distinctly present) been deemed capable of reference to some general or diathetic malady; of which, nevertheless, such lesion has evidently been the result, as proved by examination after death. These changes of organs are sometimes found to be such as we should otherwise have considered as the consequence alone of continuous diathetic mischief in the far-advanced adult. But a few weeks back, we examined the body of a girl, eleven years of age in whom the aortic valves were loaded with such an amount of semi-ossific, semi-calcareous deposit, that we never saw surpassed by the most gouty or rheumatic aged patient; and we have witnessed in a child, three years old, the entrance of the larynx constricted to a very narrow opening, surrounded by large warty excrescences or cauliflower vegetation, and which appeared to have existed for some time, judging from the phreno-glottism and laryngeal symptoms which had been long the source of great inconvenience to the little patient. In the former case, though careful inquiry was made into the probable antecedent and diathetic disorders, the child did not appear to have betrayed any morbid symptoms; until the assumed effects of the cardiac mischief began to evince themselves; no rheumatic, urinary, or inflammatory affection had been present. The first symptom that attracted notice, was the occurrence of epileptiform convulsions, due, apparently, to the disturbance of the cerebral circulation, which was, in its turn, owing to the disorder of the heart itself, uncomplained of or unknown until such convulsions had ensued.

The tendency in the child to the diffusion of morbid acts at one and the same time, or to the supervention of local disorder upon disorder, gives rise to a numerous series of its secondary diseases; but by which is nevertheless evinced the general constitutional or diathetic base at the bottom of so many of them.

"The tuberculous diathesis is a type of such; everybody now knows, that in children general tuberculosis is far more commonly met with than the local forms. The same may be affirmed of the catarrhal diathesis. The local inflammations, the result

of the latter, rarely continue isolated; a child attacked by coryza, is soon affected with bronchitis, then with pneumonia, and enteritis may even succeed to this." (p. 25.)

Analogous relations to some general cause are, in all probability, indicated in those cases where convulsions alternate with essential paralysis; and spasm of the glottis coincides with contraction of the extremities. As opposed to such instances where important symptoms have been present, and yet no adequate lesion appears to have existed, may be adduced those in which certain usually well-marked symptoms are wanting, where some lesions nevertheless exist.

"Thus we witness eruptive fevers without prodromata, pneumonia without cough, extreme lesions of the intestines without pain or with but slight diarrhoea, and important acute or chronic cerebral alterations, whose existence had never been suspected. Facts like these appear to contradict an opinion we before expressed, as to the facility with which reaction ensues in infancy. . . . But so long as the normal physiologic tendencies remain undisturbed, the relations between the lesions of organs and the local symptoms are far more exact than between such lesions and the general or reactional symptoms." (p. 20.)

As on the subject of the want of accordancy between the reaction and the lesion we have already touched, when reviewing the work of M. Bouchut, we need not dwell further upon it here. The necessity of recognising the general constitutional & diathetic basis of the local manifestation, when the treatment has to be considered, is thus alluded to by the authors:

"Those practitioners who perceive in the greater number of the diseases of childhood only local morbid conditions, and who disregard the general state, expose themselves to many reverses. Even in primary diseases, a period arrives when the latter holds sway over the former; and when the local affection should be entirely disregarded, whilst the general condition claims all the attention of the medical attendant. We may, without fear of being contradicted by experience, apply the following proposition to the greater number of the diseases of children—if taking for your only guide the material lesion, you always attack the malady with the same remedy, you may lessen it in one instance and exasperate it in another." (p. 62.)

Simply to say that a local inflammation exists, or even to indicate the organ or tissue affected, are but very slight helps in the present day towards the establishment of a valid therapeutic. In truth, we may in one sense affirm that we are yet ignorant of the disease the patient is affected with. Very many, if not most,

"Local inflammatory lesions are but the consequences, the expression, the external form of a pre-existing general condition inappreciable to sense. . . . Each time the existence, seat, and species of inflammation is established, there yet remains its nature to be determined. . . . The local inflammation, then, is not a complication of the scrofulous, variolous, or inflammatory states, it is the direct expression of them. Further, these inflammations, different in nature, are different also in their characters and course, even when their seat is identical." (p. 82.)

When discussing the subject of pleurisy (vol. i. p. 580), and after alluding to the very different material lesions met with in various forms of the malady, and the variable symptoms accompanying them, the authors inquire if such differences be not sufficient to establish the existence of diseases of quite opposite characters, having, indeed, nothing in

common but their localization in the pleura, and their "deceptive name of inflammation?" (581.) This opinion may be considered as receiving support from the consideration, that if, on the one hand, pleuritis may accompany idiopathic inflammation of the pulmonary parenchyma, appear to participate in its nature, and be cured *pari passu* with it; on the other, it becomes developed under the influences of the rheumatic diathesis, in connexion also with scarlatina, and with albuminuria; and moreover, in certain instances, appears to owe its origin to some obscurely recognised but grave modification of the economy, whose chief result seems to be, effusion of pus within the cavity of the pleura. In the present state of science, however, MM. Rilliet and Barthez do not feel justified in describing pleuritis otherwise than as a simple inflammation of the serous membrane, seeing that neither the number nor the characters of the different varieties of the affection are yet satisfactorily determined, and

"Because, in fine (and this is of extreme importance), in the same child the inflammatory affection may unite itself to the rheumatic or any other diathesis, in order to give rise to a pleurisy which shall participate more or less of one or other of the causes which gave it birth. In the same way, we have observed the catarrhal condition become united with the inflammatory, giving rise to diseases intermediate between inflammatory pneumonia and bronchio-pulmonary catarrh. Moreover, if it is at present impossible to lay down the characters of simple pleurisies, how much less can we describe those of pleurisies which are complex?" (vol. i. p. 582.)

A very marked feature of the present teaching of our authors is the important sway which the catarrhal condition is regarded in playing, as the general or constitutional cause of very many important local maladies. It seems to be viewed almost as much in the light of a distinct entity as the rheumatic or the gouty, syphilitic, and tuberculous diathetic conditions.

"Three principal causes overrule its development. One is to be sought in the individual constitution; another in the epidemic state; and the third in anti-hygienic influences, particularly those connected with deficient alimentation." (p. 82.)

At pp. 82, 3. and 4, the reader will find the nature of catarrh discussed rather in detail, but it is still more fully entered upon when the catarrhal affections of the bronchio-pulmonary structures (473) and of the alimentary canal (718) come under review. MM. Rilliet and Barthez conclude, that a fundamental difference exists between the catarrhal diseases of the respiratory and digestive organs, and the simple inflammatory ones of the same structures. Their anatomic lesions, their seat, and also their symptoms, are held to be sufficiently indicative of this. But the catarrhal maladies of the different systems belong to the same "pathologic family" (p. 717), having at their bottom, and as their general cause, the same constitutional disorder; the symptomatic differences which exist, whether of a local or general character, depending rather upon the nature of the organ in which the malady has become localized, than upon a difference of cause.

"There exists a modification of the whole economy, which holds under its dependency the changes of the fluids and the lesions of the solids of the body. . . . As to what is the nature of this general morbid condition, we are as yet ignorant. It escapes us, and will, without doubt, always elude our investigations, as does everything bearing relation to the essence of life. Nevertheless, we distinguish it from all other affections by its effects, and accept the name of catarrh, which has

been bestowed upon it on account of its most frequent manifestation—viz., mucous hypersecretion; this hypersecretion being, as we have just said, the result of the elimination, by means of the follicular apparatus, of morbid matter contained in the blood." (p. 718.)

It must be borne in mind that the authors do not deny the frequent alliance of the catarrhal and inflammatory elements, and this in all possible degrees (474); nor do they, whilst insisting on the essential influence of a general cause in the production of the former, deny the operation of "local topical ones" (p. 473). But "it is certain that this local cause is often nothing but the occasion of the localization of a catarrh otherwise imminent." Under the head of "albuminous nephritis," we marked for extract the following:

"We find again, then, in relation to Bright's disease, a new application of the humoral theory we propounded when speaking of catarrh. Here it is no longer the mucous membranes, but the kidneys, by which the elimination of morbid matters ensues. The difference of organs does not interfere with the justness of the theory." (vol. ii. p. 56.)

With regard to the pyrexial condition or fever accompanying, to a greater or less degree, the local manifestation of the catarrhal malady, and which is believed by the writers to include many of the forms of the mucous, remittent, gastric, pituitous, and other fevers of systematic authors, we condense the following opinions from what is scattered through the work:—"In the endeavours made to eliminate the morbid matter from the blood, certain circumstances may solicit the localization of such endeavours at the bronchio pulmonary organs (p. 408), at the gastro-intestinal surfaces (p. 655), at the kidneys (vol. ii. p. 56), or at the skin (p. 83). If such elimination ensue easily and quickly, the fever is seen to be apparently, subservient to the local lesion or disturbance, and is apt to be considered as symptomatic of it (p. 728). But there are reasons for believing that the fever is not constantly and necessarily merely *reactional*, but that it must be often regarded (like the local mischief) as a direct symptom of the general modification of the economy, denominated catarrh (p. 475). If, however, such elimination is unsolicited by secondary local circumstances, if no special organ become attacked, then the pyrexial movement is witnessed, *per se*, as a general functional febrile disturbance, and betrays its relations to, and its direct significance of, the general constitutional diathetic disorder (p. 729).

It is but justice to an eminent writer in this country—viz., Dr. Copland—to draw the reader's attention to the article "Catarrh," in the first volume of the 'Dictionary,' where the germs of some of the later views on the subject before us are undoubtedly to be found.

We must now pass from catarrh to diphtherite. This "is a modification of the economy, producing a local inflammation, and impressing it with a stamp peculiar to itself." (p. 81.)

"When the inflammation is propagated to the larynx, the prognosis is that of croup. When false membranes are developed on particular portions of the teguments, if the diseased surfaces become covered with thick deposits and furnish abundant suppuration—if the inflammation becomes scirpiginous—if erysipelas increases its extent—the prognosis becomes infinitely grave, and death may follow in the midst of profound adynamia. . . . In other cases the mucous surfaces, which are in contact with the atmosphere (anus vulva) become covered with

diphtheritic false membranes, which, by their progressive extension, determine the death of the patient." (p. 258.)

The diphtheritic may become combined with the catarrhal condition, and by the alliance give rise to certain modifications of the ordinary character of the former. M. Pidoux* has gone to the extent of basing a new affection on these changes, and which he calls *plastic catarrh*. Upon the subject of diphtherite, we may refer to some remarks in our eighth volume, page 69, as not inappropriate to be here reconsidered.

We need scarcely do more than recal to the recollection of the reader how largely the rheumatic and gouty diathetic conditions have of late years been appealed to, as regards the genesis of numerous forms of local disorder in the adult frame, to make it appear not unlikely that the former state at least might be called upon to resolve certain topical manifestations occurring in earlier life. And so it will be found: for independent of the typical articular disorder, certain forms of essential paralysis, of inflammation of serous membranes, chorea, and even eclampsia, have all been regarded as local effects of the rheumatismal disorder. Long ago, Stöhl and Bouteille, and more recently, Bright, noticed the coincidental occurrence of rheumatism and chorea. Still later writers have repeated the observation, and sought to establish a causal connexion between the two. But it is more particularly in the writing of Secl and Botrel† that the doctrine of most intimate relationship is taught. The point meets with close consideration from our authors, but we can only spare room for the following *résumé*:—

"The proofs of it must be sought in the affinities of coincidence, of causation, of symptoms or cadaveric changes, and of treatment." (p. 586.)

Inquiry so conducted leads to the opinion that such proofs of relationship are wanting, or at any rate

"We will say that choreo-rheumatic coincidence is the only proof that can be brought forward to establish a connexion, as regards *nature*, between the two maladies; that the frequency of this coincidence appears to have been exaggerated; and if we refuse not to admit that chorea may be one of the manifestations of rheumatism, we yet await other proofs before giving in our entire adhesion to this opinion." (vol. ii. p. 587.)

When noticing the 'Treatise' of M. Bouchut,§ we expressed our belief that *some* varieties of "essential paralysis" would be found to be rather of a *myogenic* than nervous character, and coincided partly as regarded the rheumatic doctrine so strongly insisted upon by this writer. With respect to muscular contractions, the "external tonic convulsion" and "spasmodic retractions" of some authors, we may observe that many pathologists, especially Corvisart, Delpech, Secl, and De la Berge, have dwelt very strongly on their connexion with the rheumatic diathesis. We confess that we have not been able to arrive at a very clear interpretation of MM. Rilliet and Barthez' views on this matter. At first, we considered them as regarding the malady "as of the same nature as

* Journal de Médecine, 1843, p. 154.

† De la Chorée, Mémoires de l'Acad. Nat. de Médecine, tom xv.

‡ De la Chorée considérée comme Affection Rhumatismale, Paris, 1850.

§ British and Foreign Medico-Chirurgical Review, vol. xii. p. 111.

the convulsions of young children," next as "a neurosis of a rheumatic character, and lastly, as

"Not refusing to admit two species of contraction—the one purely rheumatic, the other connected with a functional perturbation of the nervous system. The first is frequent in the adult, the second in the child, occupying especially the extremities. As to the similitude established between the contractions and intermittent fever, it appears to us superfluous; the rheumatic element amply suffices to render account of the intermissions." (vol. ii. p. 497.)

We admit that we were but little prepared for the following views in "connexion with sclerema:—

"In reflecting on the nature of this rare and curious malady, we are strongly inclined to regard it as rheumatismal. Our first case supports this opinion, for the complications which were successively developed had their anatomic seat in the mucous membranes, and we know that the rheumatic diathesis specially affects these membranes. The child forming the subject of the second example had been subjected to the influence of atmospheric conditions which engender this malady: she inhabited a damp lodging. . . .

"If the opinion we thus venture on be confirmed by other observers, sclerema will be another malady to be removed from the group of local affections, and associated with a diathesis." (vol. ii. p. 114.)

In the course of some preliminary observations on the "hæmorrhages," occur the following remarks worth quotation:

"This would undoubtedly be the place to inquire if there exists an hæmorrhagic affection or diathesis analogous to the serous diathesis, to the inflammatory, &c. fatal conditions, all general maladies whose existence we have admitted; but the elements for resolving this question are wanting." (vol. ii. p. 238.)

The modifications of the whole economy which may give rise to the *neuroses* are, according to MM. Rilliet and Barthez, of three kinds: the nervous condition, properly so called, the rheumatic, and the cachectic. Badly defined as the first is as an alteration of the general health, yet its existence is widely admitted. Indeed, it is rather by exclusion than by a direct method of reasoning that its specification is arrived at. Very many of the *neuroses* have not, therefore, the same origin; and, as regards children, it is important to distinguish those nervous affections which are symptomatic from those which may be regarded as idiopathic in their character. The latter are the disorders particularly included under our present head by the authors, who define them as *apyrætic maladies*, characterized by a disturbance of the functions of the nervous system, which latter does not present any appreciable material lesion (vol. ii. p. 448). M. Racle has strongly advocated the doctrine,* that the affections having their seat in the encephalon are frequently associated with some general or diathetic disorder. Thus the greater number of meningitic diseases, certain hæmorrhages, and some forms of hydrocephalus, are dependent upon tubercularization. Simple primary meningitis, however, is often associated with rheumatism, some cerebral hæmorrhages with purpura, and certain hydrocephalic affections with an albuminous condition of the urine. The opinions of MM. Rilliet and Barthez substantiate, in the main, these views; but they admit that, during the first months of life, we frequently observe very serious cerebral symptoms, sometimes of a convulsive, at

* *Récherches sur les Affections du Cerveau dans les Maladies Générales.*

others of an ataxic, character, rapidly terminating in death, and revealing no sensible lesion of the cerebro-rachidian axis or rest of the nervous system, no alteration of any other organ under the most careful investigation, nor the existence of any other malady, through whose medium such cerebral symptoms have become developed (p. 93).

The propriety of maintaining the distinctions between primary and secondary diseases is emphatically dwelt upon by the authors; and the important influences which acute secondary affections have in producing the *cachectic condition*, and *vice versa* the peculiar character impressed on such affections when occurring in cachexia, are severally treated of. This general cachectic condition is often the result of a vicious hygiene, and is thus what may be termed primitive in its nature, and the certain origin of a numerous series of morbid symptoms and manifestations of local disorders. The younger the child, the more readily is the condition of cachexia by such means induced, as well as being the rapid and essential consequence, not only of secondary diseases, but sometimes even of primary ones, too. In such cases,

"The organs have lost all power and resistance. Vital activity is nearly abolished, the child does not develop, it vegetates; and like the aged, it inclines towards the grave by excess of organic and vital debility. The energy of the power of growth is destroyed, or at least temporarily suspended; for the elongation of the body, which we were speaking of not far back, when alluding to feeble disorders, cannot here be verified. Further, if this condition persists, and if by a happy exception the child recovers, years pass, in which it very slowly develops, and retains the figure and bodily appearance of a much younger child, a condition contrasting remarkably with the aged aspect of its countenance" (p. 28.)

Independent of certain local and traumatic *gangrenes*, the children, more particularly of the lowest classes, and of some densely-crowded hospitals on the Continent, are well-known to evince the death of parts and organs under conditions which leave no doubt as to the intimate alliance of such gangrene with some general constitutional or diathetic affection. As to the nature of the latter however, we are ignorant. That it belongs to the category of the cachectic diathesis is undoubted, but there seems to be something superadded to mere debility or impoverishment of the blood, whether by anti-hygienic or pathologic causes. It is difficult to believe that there is not another element in play along with cachexia, besides the "scorbutic" of MM. Bouley and Caillaud, the "defibrination" of M. Boudet, and the "augmentation of alkalis" of M. Becquerel. We have reason for thinking that gangrene of the lung, of the mouth, and of the pharynx, are associated with some absolute general and extreme vital deterioration not as yet definitely signalized. One remarkable fact may be here stated—viz., that out of one hundred cases of gangrene, forty were associated with measles (Tourdes Du Noma, &c.) According to our authors:

"The general gangrenes become developed under two influences:—

"1. That cachectic or scorbutic state, almost special to children, resulting on the one hand from the anti-hygienic conditions under which they live, and on the other from the prolongation or the succession of diseases they suffer.

"2. A special condition, determined by certain maladies, and particularly by measles." (vol. ii. p. 313.)

The concluding portion of the second volume of the 'Treatise' is

occupied with the consideration of the "General Acute Specific Maladies," (p. 599). Of these, parotitis, pertussis, and typhoid fever, are now discussed; while variola, vaccinia, scarlatina, and measles, are retained for the third and concluding volume. The latter, however, it is presumed, will be chiefly taken up with the important subject of *tuberculosis*.

It is unnecessary here to dwell upon the proofs of the existence of some general modification of the whole economy, some special change of the dynamic or constituent states of the circulating fluid, which form the basis of the external manifestations of the febrile specific maladies; and as the subject of tuberculization, whether in its diathetic or local forms, is as yet unreached by our authors, we need not dilate upon it now. In conclusion, we would observe that whilst the 'Treatise' strikes us as having somewhat a less prolix character than before, there is yet much diffuseness, and even repetition of details in it. We doubt, too, whether, for particular purposes, the more compact though superficial volume of M. Bouchut will be displaced by it; though there can be no question that it will continue to occupy its position in the judgment of the most worthy as the chief authority, the first systematic work on the increasingly important subject of *pædiatrics*.

W. Hughes Willshire.

REVIEW V.

1. *A Manual of Pathological Anatomy.* By C. HANDFIELD JONES, M.D., and EDWARD H. STEVENSON, M.D.—London, 1854.
2. *Handbuch der Speciellen Pathologischen Anatomie.* Von Dr. AUGUST FORSTER.—Leipzig, 1854.
Manual of Special Pathological Anatomy.
3. *Atlas der Mikroskopischen Pathologischen Anatomie.* Gezeichnet und Bearbeitet von Dr. AUGUST FORSTER.—Leipzig, 1854.
Atlas of Microscopical Pathological Anatomy. Observed and Drawn by Dr. AUGUSTUS FORSTER.

EVERY work treating of the science of medicine ought to be historical, original, suggestive. The amount of information given under each of these headings must vary according to the extent, character, and tendency of the subject. In a dictionary or cyclopædia of medicine, for example, we should find a full historical account, with more or less original matter; in a monograph, the original is expected to predominate; while the suggestive appears to find its most appropriate place in pamphlet medical literature.

The class for whom a work is intended forms another very important consideration in book-writing, a consideration which, in general literature, is never lost sight of by either author or publisher. Hence, our opinion cannot be truly valuable unless we are familiar with the educational or social position of the individuals among whom a work is to be circulated, and upon whom its influence is to be exercised. These are our reasons for believing that in literature, as in art, each work should be examined from the point of view indicated by its author.

Simple as are these considerations, they have, we believe, been too frequently overlooked, or have not suggested themselves to the majority of reviewers of scientific books—an observation that is particularly applicable to our Continental brethren. They should remember that the points from which we started are far apart. Many of the branches of science which they have been for many years investigating and teaching, are in this country examined by but a few, and are considered as accomplishments rather than requisites in a medical education.

Before we contrast the investigators, authors, and teachers, of medical science in Great Britain and Ireland, with their fellow-labourers on the Continent of Europe, it ought to be recollected that, in this country, the pioneers of medicine have to work individually, and without assistance. On the Continent, the moment a few master-minds have perceived that medicine is about to take a new course, which they consider likely to lead to discovery, they have but to touch the well-oiled medico-educational machine, when it instantly turns in that direction, and the new science finds a professor whose salary is sufficient to enable him to devote his time and attention to the advancement of his particular department.

How different is this position from that of the men who live in our great cities, where toil from morning until night obtains merely a sufficiency of the necessaries, not to mention the civilization-created luxuries of life, without which their position in society cannot be maintained! The saddest part of this statement consists in its simple truth. Let those who doubt it go to each village and town and church-yard in these islands—there they will find our witnesses—men who have sunk under the struggle for life, and yet have left behind them the signs of good done in evil days—men who still hold out in the daily conflict between necessity upon the one hand, and the feelings of educated men upon the other—men who, while their services cannot be dispensed with, are harder worked and worse paid than the mechanic who stuffed the parliamentary seats of those right honourable gentlemen that “take advantage of a poor but truly honourable profession.” Before we calculate how much love for science exists throughout the practitioners of this country, it must be borne in mind that we are all obliged to be practitioners: by practice we must live, and from it are snatched the few hours we can devote to study and research. These considerations justify us in claiming the highest praise for those who are endeavouring to form a taste and a school for pathological and rational medicine in this country; and we receive the ‘Manual of Pathological Anatomy’ as a proof that this science is advancing with a force too great to be long resisted by the curricula of examining boards.

“The absence of any original work in the English language which embraces the entire subject,” is the reason given in the preface for the authors’ having undertaken such a work. This not only renders such a book absolutely necessary, but also appears to us to have considerably increased the labour and difficulty of its composition. In the table of contents, the name of each author is attached to the division of the subject he has described; and as they “are each individually responsible for the chapters they have treated,” we shall examine the work in the order they have adopted.

"Some general observations must, however, be premised," which consist in succinct explanations of the terms—"Morbid anatomy, pathology, exciting and predisposing causes, idiosyncrasies, diathesis, degeneration," &c. With respect to the long-debated question, whether diseases had their principal seat in the fluids or in the solids of the body, Dr. Jones observes, that—"At the present day we marvel how men could have adopted exclusively one view or the other, and refused to each their share in the production of morbid phenomena." This is the result of modern pathological investigations, to which belongs the merit of having swept away a heap of errors of the worst kind, inasmuch as they were the source of the most destructive bed-side practice.

At this, the very outset of our review, we must object to all vagueness of description in pathological science—therefore we cannot allow the following sentences to pass unnoticed: "That, on the one hand, there is a sound and healthy systemic life—there often comes in its stead, either generally or locally, an unsound and unhealthy life, which leads to perverted morbid action, or to actual decay." (p. 5.) This is given as "A great practical truth, which daily experience presses on our attention." Now, we refuse altogether to discuss this proposition, as the terms "healthy life," "unhealthy life," "perverted morbid action," are too indefinite to admit of argument. Criticism of this kind may, perhaps, seem little more than mere word-catching; but if our ideas are well defined, it is an easy matter to find words in which to express them. In a word, it is better to avoid all generalizations and explanations, than employ words that are neither precise nor self-explanative. Professor Engel, of Prague, has directed particular attention to the want of accuracy of expression in medical reasoning, and we hope to see these and similar inadvertencies avoided in succeeding editions of the 'Manual.'

The second chapter contains a highly practical and pretty complete expression of the researches of most of the authorities upon the normal and abnormal conditions of the blood. The contrast drawn between the descriptions of the textural characters of fibrine, as described by Rokitsansky and Paget, is readily understood and instructive. The organic constituents of serum, extractive oily matter, and the saline ingredients in the blood, together with the parts that water, urea, bile, lactic and oxalic acids, play in disease, are fully described. Dr. Jones observes, that it seems very doubtful that such a condition as partial *anæmia* can exist, "if we recognise a deficiency of red corpuscles as an essential feature of this state." In general hyperæmia, on the other hand, Rokitsansky is of opinion that there exists a qualitative as well as quantitative change in the blood, resulting from imperfect oxidation: while local hyperæmia is accompanied by but a quantitative change. This distinction does not appear to us to be well founded, as it is more than probable that the elimination of certain ingredients from the blood must be subject to a great variety of circumstances; and that the same amount of these changes cannot occur in the liver, for example, when in the normal, in the anæmic, and in the hyperæmic states. If blood, then, passes through an organ without undergoing the usual changes, it contains abnormal ingredients, and is therefore qualitatively changed. We are consequently inclined to believe that hyperæmia, whether general or local, is accompanied by a qualitative as well as quantitative change in the blood.

Adopting Dr. Williams's arrangement, our author considers local hyperæmia under—*first*, hyperæmia with diminished motion of the blood in the part; *secondly*, active hyperæmia or determination of blood—meaning thereby a superabundance of blood resulting from increase in the velocity with which the blood is supplying a part; and *thirdly*, inflammation, or that variety of the hyperæmia in which the movement of the blood is partly increased, partly diminished. Regarding the practical and scientific value of this arrangement, Dr. Jones observes:

"It is not certain, especially as respects determination of blood and inflammation, that it is correct, or rather, that it may not prove a source of error by not being founded on that which is the essential circumstance in these two conditions. This, at present, is not possible, from the imperfection of our knowledge; but it may be well to bear in mind the above caution." (p. 71.)

Hæmorrhage, flux, and dropsy, are given as the results of hyperæmia. Respecting the former we read, that

"In every case where blood is effused in any quantity, the walls of the vessels must have given way; and perhaps this is the case in every instance where a blood-globule escapes from its channel, though it is not, to our minds, absolutely certain that there is no such thing as the hæmorrhage by exhalation of the older writers." (pp. 55, 59.)

It appears self-evident that there are but two ways in which a fluid, having physical characters such as blood, can escape from the vessels—*first*, by a solution of continuity in the tube; *secondly*, by filtration through the tissues of which the tube is composed. The causes of the first of these states are well known; the second, the filtering of blood-corpuscles through the walls of vessels, cannot, we believe, occur until two changes have been effected, both resulting from the abnormal state of the blood which causes hæmorrhagic diseases—*first*, the walls of the vessels having been badly nourished, are consequently lax; *secondly*, the fibrinous glaze which, in the normal condition, is being constantly laid down on the innermost surface of vessels of all sizes, out of the blood circulating in the calibre of the tube, is no longer well formed—then the walls of the bloodvessels are in a condition similar to that of unglazed paper, through which the corpuscles can readily pass.

We offer this explanation of spontaneous hæmorrhages, believing it to be of a physical, and perhaps even demonstrable character.

Relative to the injurious effects of hæmorrhages it may, we believe, be laid down as a rule, that they are to be estimated by the position, rather than by the amount of blood escaped.

Dr. Jones agrees with Mr. Paget and Wharton Jones in rejecting Dr. Williams's opinion, "that an essential part of inflammation is the production of numerous white globules in the inflamed vessels; and that the obstruction of these vessels is mainly due to the adhesive properties of these globules;" while he differs from Mr. Paget, and agrees with Rokitsansky and Williams, "as to the increased production of white corpuscles in the inflamed vessels." Were we obliged to give an opinion upon the subject, we would agree altogether with Mr. Paget, and refer the increased number of colourless corpuscles seen in the vessels of an inflamed part, to an accumulation, rather than a local production, of white corpuscles. We are well aware that the elements of these corpuscles

are constantly present in the blood; and the increased stimulus given to development by the local inflammation may exercise an influence upon these elements, and cause their rapid development. Yet, however, from actual observation, we are inclined to refer their increase to accumulation.

"We saw reason to believe that the tissues, in virtue of their nutrition power, exercised an influence on the movement of the blood; that in active hyperæmia their attractive force was increased; and we would now add, that it is through the failure of this nutrition power that we believe stagnation takes place. . . . The nutrition power of the tissues is chiefly concerned in the production of the flow of active hyperæmia and the stasis of inflammation." (p. 109.)

Under "Inflammatory Exudations," and "the changes that take place in the tissues affected by them," the author, following Rokitsky's description, differs from him concerning the albuminous exudations, which Rokitsky considers to be distinct from the fibrinous; a separation Dr. Jones considers to be scarcely warrantable.

The differences between pus and mucus are given thus:

"Liquor puris is albuminous, liquor mucus not so; pus will mix with water, and mucus will not; pus is dissolved, in some measure, by acetic acid, while mucus is coagulated; and mucus generally contains traces of epithelium, while pus does not. It is manifest, from what has been stated, that the difference between mucus and pus consists essentially in the different nature of the fluids, not in that of their corpuscles." (pp. 127, 128.)

Under "Leucocythemia," we find Dr. Bennett's is the only name mentioned. It must be borne in mind that Professor Virchow has not only thoroughly investigated this condition, but was also the first that recognised the true nature of the corpuscles, which, from Dr. Bennett's first case, were believed to be pus cells.

The complete adoption of the term "*crasis*" must not pass unnoticed. "*Crases*" have been described by Rokitsky as so many "*alterations of the natural composition or mixture of the blood*;" and we find the section on tubercle headed, "The Tuberculous *Crasis*—Tubercle." We would much prefer the word "*state*," as the term "*crasis*" seems to convey the expression of a substantive change in the blood, to which disease can be referred, and by which its phenomena may be explained. The doctrine of "*crasis*" was very useful in its time. Many morbid changes received a more or less complete explanation by reference to this theory; but we have no positive knowledge of the changes in, or conditions of, the blood, to which the term has been applied. In fine, the phrase, "*crasis of the blood*" belongs to that period in the history of pathological anatomy that has been rendered an epoch by the publication of Rokitsky's work; but it is in reality merely a speculative one, and has not been generally received by pathologists.

"Textural Changes," including hypertrophy, atrophy, induration, and softening, together with the fatty, fibrous, and calcareous degenerations, are next described.

The "*degenerations*" are defined as "*changes of an essentially chronic character, latent in their origin, and obscure in their progress, until they have produced such deteriorations of structure as give rise to prominent secondary phenomena*." (p. 154.)

Fibrous degeneration has been lately fully described by one of the

authors of the 'Manual' in this journal. It is stated to be somewhat allied to induration, but to be distinguished from it by there being scarcely any exudation in the fibrous degeneration; or exudation takes place slowly, and at once passes into fibre. In induration, on the other hand, "a notable quantity of blastema is effused, which compresses and atrophies the adjacent texture." (p. 154.)

Under "New Formations," the author follows,

"In the main, the arrangement which Rokitsansky has adopted: sets forth their distinctive features, as far as possible; and recognises the frequent insufficiency of any structural or chemical peculiarities, to explain or even diagnose the essentially different natures of different specimens we may meet with."

Conformably with the course we have adopted in a former review,* this part of the subject shall be noticed in an article devoted to the consideration of "tumours;" nor will this delay detract from the completeness of our present analysis, as Dr. Jones's articles in this Journal, 'On New Formations,' have, we feel assured, convinced our readers that he is quite familiar with these growths; while, before closing the "general" part of the 'Manual,' it is only necessary to add, that those who desire to ascertain the leading characters of the vegetable and animal organization that exist in and upon the human body, will find the chapter on "Parasites" highly interesting and instructive.

The succeeding 572 pages, treating of "special pathological anatomy," do not admit of so detailed an analysis as that just given of the division, "General Pathological Anatomy." We shall, however, notice its most salient parts, and refer at intervals to Förster's 'Manual.'

"The Nervous System," described by Dr. Sieveking, opens with some general observations regarding the physiology of the series of organs about to be examined. The author proceeds to describe the pathological anatomy of, *first*, the brain and its membranes; *secondly*, the spinal cord and its membranes; *thirdly*, the cerebro-spinal nerves; *lastly*, the sympathetic system. The lesions of the dura-mater, of the arachnoid, and pia mater, with the changes which occur in its vessels; the lesions described under the names of tubercular meningitis and hydrocephalus, with that interesting class of tumours found in the choroid-plexus, to which Virchow has given the name of "corpora amyacea," and Dr. Jones of "concentric corpuscles," form the subjects with which this division is introduced to our notice.

"It is highly desirable that anatomists should settle its (the arachnoid's) normal relations, in order that the deviations occurring in morbid processes may receive a correct and proper estimation."

Prof. Kölliker has evidently represented the true state of the case when he observes,†

"Hence has shown that the arachnoid is not a serous sac, as is the pleura, or pericardium. Its inner surface, with its epithelium, is everywhere in close contact with the dura mater, so that a *cavum arachnoideum* does not in fact exist."

Quoting from the same authority, Dr. Sieveking observes‡

"Those who state that the arachnoid lines the ventricles of the brain, and the processes of the pia mater contained in them, suppose a thing that is impossible—viz., that the arachnoid passes through the pia mater, and invests the surface of the plexuses, which is actually an internal one."

* The Pathologico-Anatomical School of Vienna. No. xxviii., 1854.

† Mikroskopische Anatomie, von Dr. A. Kölliker, Band ii. Hälfte 1, § 1.

That the students of our schools are disposed to take a very great deal as granted, and also that the teachers of anatomy are well inclined to consider as proved all that relates to ordinary dissecting-room anatomy, must be evident to those who are acquainted with the present system of anatomical instruction. If our normal anatomy be wrong, our pathological descriptions must be very far from truth. If we had as little faith and as much industry as our German colleagues, it would have been unnecessary for us to quote a second time from our ever-working friend Dr. Kölliker, who, with Virchow, Scherer, and Scanzoni, has made the little town of Würzburg equal, if not superior, as a scientific school, to any in Germany. Dr. Kölliker writes:*

"With respect to the *pia mater*, very many authors assume that the *placae chorioidei ventriculi tertius et quartus* are connected with each other through the *aqueductus Sylvii*; further, that the *ventriculus quartus* is not completely closed, and that the *calamus scriptorius* communicates by a foramen with the subarachnoidal space. Both of these assertions are incorrect, as is that which affirms that a particular serous membrane lines the ventricles."

In the succeeding chapter "On the Brain," Dr. Sieveking discusses the question, whether the amount of blood contained in the cranium of the adult can vary? Physiologically and pathologically important as it is that this question should be satisfactorily and fully answered, we do not at present feel in a position to enter fully into the discussion it has excited. We shall therefore state the negative opinions in the words of Dr. Hughes Bennett:†

"But if we imagine that tenesection will enable us to diminish the amount of blood in the cerebral vessels, the theory (held by the Edinburgh school) points out that this is impossible, and that the effects of bleeding are explained by the influence produced on the heart, the altered pressure on the brain, exercised by its diminished contractions, and the change of circulation within the cranium thereby occasioned."

This sentence contains the admission, *first*, that the diminished action of the heart can alter the pressure on the brain—as the consequence of more or less blood being sent to that organ—the brain then can be thereby compressed, or that pressure can be diminished;—and *secondly*, that a change of circulation can be thereby occasioned.

Dr. Bennett further states, "there are many circumstances, however, which occasion local congestions on the brain, in which case another portion of its substance must contain less blood." To this assertion the following experiment, made by Berlin under Donder's direction, affords the best reply:

"A portion of the skull of a rabbit was removed, the corresponding piece of dura mater cut out, an accurately fitting portion of a watch-glass let into the opening in the calvarium, and the junction made air-tight with gum. When, by compressing the nose and mouth, respiration was intercepted, within ten seconds the increased redness of pia mater could be seen with the naked eye. This condition was made still more evident by the use of the microscope; and each time some minutes elapsed before the congestion again diminished. A dependent position of the head also increased the hyperæmia. Rapid abstraction of blood very distinctly diminished the diameter of the vessels."‡

* Mikroskopische Anatomie, loc. cit.

† Lectures on Clinical Medicine, p. 143.

‡ Die Pathologie und Therapie der Gehirn-krankheiten, von Dr. Leubuscher, zweite Abth. 8. 185. Berlin, 1854. The experiment appeared originally in *Nederlandsch Lancet*, p. 461, 1850.

Until we have experimentally investigated this matter, we refrain from giving an opinion respecting it, though we feel disposed to agree with Dr. Sieveking, "that a variation does take place, and sufficient to account for many of the phenomena of nutrition and disease;" we shall, however, avoid "confounding blood with fluid, and brain with cranium," an error into which, in Dr. Bennett's opinion, Dr. Burrows has fallen.

Congestion of the brain, hæmorrhage on its surface, into its substance and ventricles, with white and inflammatory softening, œdema, induration, hypertrophy, and atrophy, is the order adopted by Dr. Sieveking. Förster describes the anomalies in the size, position, &c., of the brain, the results of wounds, the new formations which occur in it, inflammation, softening, and induration; the different forms of abscesses; hyperæmia, anæmia, hæmorrhage, œdema, and, lastly, the parasites found in the brain, as the *œctinacoccus* and *cysticercus cellulosa*. Förster mentions three kinds of softening, red, white, and yellow; in the latter, "the clear yellow colour is caused by a diffusion and alteration of the colouring matter of the blood." Rokitsansky was the first who drew particular attention to this variety of softening; he observes—

"Blood-corpuscles and the pigment already alluded to have as little to do with the production of the yellow colour as purulent matter, for the fluid contains far too small a quantity of blood-corpuscles, as well as of amorphous pigment, to account for it. My own impression is, that yellow softening is founded in a chemico-pathological process."*

Rokitsansky alludes to Fremy's researches, which we cannot notice further than that he considered one of the constituents of the brain to be oleo-phosphoric acid, free, and in combination with soda; this organic acid is, at ordinary temperatures, when brought into contact with water or with decomposing animal matter, converted into phosphoric acid and olein; the olein is rapidly converted into oleic acid, which forms a soap with the ammonia; such, at least, is Fremy's view of the process of yellow softening. According to Lehmann,† the oleo-phosphoric acid of Fremy has been proved to be cerebrie acid, and the result of its decomposition, as Goble discovered, is glycerin-phosphoric acid ($C_2H_5O_5$, $2HO + PO_5$), not free phosphoric acid. In confirmation of which, Lehmann states that it occurred to him once to observe a well-marked acid reaction, such as had been already noticed by Rokitsansky, in a case of very extensive yellow softening of the brain, a reaction that was positively ascertained to be caused by glycerin-phosphoric acid.

"The absence of pus cells would not be an absolute proof that *suppuration* had not occurred; for they are not always present‡ in undeniable abscesses, where molecular granules and pyoid bodies may be the sole objects detected by the microscope."

This statement may lead to much confusion, and therefore requires comment. The absence of pus cells would, if we rightly understand the subject, not be absolute proof that *inflammation* had not occurred; the granular exudation-corpuscles being the form of cell under which the product of inflammation of the brain's substance shows itself. If *suppu-*

* Rokitsansky's Pathological Anatomy, Syd. Ed., vol. iii. p. 423.

† Lehrbuch der Physiologischen Chemie, zweite Auflage, Band iii. § 99. 1853.

‡ Lebert: Physiologie Pathologique, vol. ii. p. 303, in a note.

ration occurs—by which must be meant the formation of pus, and not simply softening of the brain—pus cells must be, and are, found in all such cases. How pus cells could be absent from “undeniable abscesses” is beyond our comprehension; as we understand an abscess to be an accumulation of pus, the microscopic character of which fluid is the pus cell. There is a mistake with respect to the authority given for this statement, as at volume second, page 303, Lebert describes “cancer of the breast,” not abscess in the brain. We have moreover read this author’s description of inflammation of the brain, without being able to find any statement similar to that just given; on the contrary, pus cells are mentioned as having been found in a case* of “cerebral inflammation, with red softening, around an encysted abscess;” this being the only case that has been given of inflammation of the brain accompanied by the formation of purulent matter.

The morbid growths described as found in the brain are tubercle, cancer, cholesteatoma fibroids, and cysts. Dr. Sieveking, treating of cancer, observes—

“There are no symptoms peculiar to the disease, beyond the effects resulting from pressure; and even they do not appear to be in any way commensurate with the size of the deposit.” (p. 261.)

This remark may be extended to all the adventitious growths in the brain, as their position and character, rather than size, regulate the production of symptoms.

Förster treats of the pathological alterations which occur in the spinal cord and its membranes, and uses headings similar to those given above, under which he has described the lesions of the brain and its membranes. After a brief description of the most important of these conditions, Dr. Sieveking “regrets that our knowledge of the morbid changes occurring in it (the spinal cord) are in no way commensurate with the importance and dignity of the organ.” The mechanical difficulties attendant upon examination of the spinal cord are in a great measure removed by the use of the double-blade saw-edged rachitome of M. Charrière, of Paris. After having repeatedly used this instrument, we can testify to its utility. With it the entire spinal cord may be laid open by an assistant in about ten minutes. Finally, those who are interested in the lesions of this organ will consult with advantage the different articles published by Dr. Türk, of Vienna,* who, as physician to the department of the “Allgemeine Krankenhaus,” for diseases of the nervous system, has ample opportunities for such study.

Förster considers the lesions of the organs of circulation in the following order: Congenital anomalies of formation, alterations of size and position, wounds and ruptures, new formations, inflammation, metastatic abscesses, hyperæmia, anæmia, hæmorrhage, pneumatosis, parasites, and cadaveric changes. With reference to the production of hypertrophy of the heart,

* Lebert’s *Physiologie Pathologique*, tom. I. p. 120.

† Ueber kompression und ursprung des schnervens.

Ueber den Zustand der Sensibilität nach theilweiser Trennung des Rückenmarkes.

Ueber ein bisher unbekanntes Verhalten des Rückenmarkes bei Hemiplegie.

These and other articles by Dr. I. Türk are published in the *Zeitschrift der k. k. Gesellschaft der Aerzte in Wien*, 1850–51; and in the *Sitzungsberichte der Mathem.-Naturw. Classe der kais. Akademie der Wissenschaften*, Band ix. § 229.

Dr. Sieveking, by whom the division "Pathological Anatomy of the Organs of Circulation" has been written, expresses his own opinion by adopting the terms used by Dr. Watson—thus: "There is something to hinder the free and sufficient play of the organ—an adhering pericardium, it may be, or mal-position of the heart." We turned to the section on "Atrophy of the Heart," expecting to find adherent pericardium mentioned among the causes of the latter affection; but this cause has been lost sight of by our English author, while Förster gives "the formation of thick fibrous masses after pericarditis" as one of the causes of this atrophy. Practitioners and students ought to understand fully the effects of so frequent a disease as pericarditis; and as it is commonly taught that hypertrophy of the heart is the result of adherent pericardium, we turn to Rokitsansky's work, translated by the Sydenham Society; there we find—

"These conditions are occasioned by the injurious influence exerted by the pericarditic process on the heart, in consequence of which the muscular substance of that organ is paralyzed, and a flabby condition induced, which admits of the texture being easily torp, and which speedily yields to (passive) *dilatation* of the heart. (p. 137.)

"1st. The mechanical obstructions which give rise, according to circumstances, either to preponderance of *dilatation* or preponderance of *hypertrophy*, are—(a) Mechanical obstructions in the ostia of the heart; (b) Mechanical obstructions in the arterial trunks; (c) Similar (mechanical) obstructions in the capillaries. . . .

"2nd. Diseases of the texture of the heart (which produce *hypertrophy* of the heart) are, &c. . . .

"3rd. Excessive innervation of the heart," &c. (pp. 162, 165, 166.)

Passing to "Atrophy of the Heart," we read—"Atrophy of the heart is, moreover, also the result of pericarditis." (p. 170.) And if further confirmation is necessary, we find Dr. Stokes, in his late work "On the Diseases of the Heart and Aorta," writes thus:

"Without denying that a general adhesion may induce *hypertrophy* and *dilatation*, experience leads me to doubt that such an effect accessarily, or even commonly, follows the condition indicated. I have often found the heart in a perfectly natural condition, with the exception of an obliterated pericardium." (p. 11.)

We know of no opinion more valuable than that of Professor Smith, whose opportunities for such investigations are extensive, while his reputation as a pathological anatomist is not limited to these islands or to the Continent.

"It has been stated to me," writes Dr. Stokes, "by Professor Smith, that he has found general adhesion of the pericardium coinciding with atrophy or with hypertrophy of the heart, in a nearly equal frequency. And it is a remarkable fact, recorded by the same observer, that he has always found ossification of the pericardium, which we may hold as the extreme of the obliterating process, attended with atrophy of the heart." (p. 12.)

Drs. Barlow,* Chevers,† Walshe,‡ and W. T. Gairdner,§ may be referred to; their observations, as quoted by Dr. Stokes, all tend to establish the rule originally stated by Rokitsansky, that *atrophy* must be considered as one of the most frequent consequences of adhesion of the pericardium; and that, in the great majority of cases in which, as a consequence of adhesion,

* Medical Gazette, 1847.

† Guy's Hospital Reports, vol. ix.

‡ Practical Treatise on Diseases of the Lungs and Heart, p. 452. 1851.

§ Monthly Journal of Medical Science, vol. xii. p. 108.

hypertrophy occurs, its production is a secondary consequence; the order of phenomena being—pericarditis; adhesion, with consequent paralysis, and often fatty degeneration of certain portions of the muscular structure of the heart; dilatations which, to use Rokitsansky's words,* "are maintained by their own secondary conditions, which mechanically augment them, and gradually superinduce hypertrophy."

Dr. Sieveking is of opinion—

"There is no doubt that, in a majority of cases, the atheromic deposit is a secondary fatty degeneration of fibrine. *Ætheroma* appears, in the first instance, as a series of fibrinous layers, subjacent to the inner coat of the artery, formed by a process analogous to secretion from the blood." (pp. 349, 350.)

A careful examination of *ætheromatous* deposits has led us to agree with Rokitsansky, and to refer the production of *ætheroma*—in many cases, at least—to the deposit of oily matter from the blood circulating in the calibre of the tube, on the innermost surface of the vessel; at the same time, we do not deny that fibrinous deposit may occur in the elastic tissue of an artery, and that fatty degeneration may arise in such a deposit, for we have too much confidence in Dr. Sieveking's accuracy as a microscopical observer to doubt the absolute correctness of his observation; though we cannot admit that, "in a majority of cases," *ætheroma* is produced in the manner he describes.

Passing to the chapters "On Aneurism," it appears to us to be a curious circumstance that our friend, Professor Förster, quotes at great length from Dr. Crisp's work 'On Diseases of the Bloodvessels,' while Dr. Sieveking's statistics are taken from the researches of M. Bizot and Mr. Hodgson. We trust that the labours of a fellow-countryman will not, in future, be lost sight of by English pathologists; it should be remembered that Dr. Crisp's work contains an account of 551 cases of aneurism, selected from British medical journals, and of 364 preparations of aneurism in the London museums; while Hodgson's cases number 63 only, and M. Bizot's but 189.

Adopting the general description of authors on aneurism, Dr. Sieveking observes, "it is unknown in childhood." Dr. Crisp has recorded one case, in which the patient was aged nine years, while five are noted by him as having occurred between the ages of ten and twenty.

The occurrence of entozoa in the blood of man and animals has attracted considerable attention, and in addition to the references given in the works before us, we would direct attention to the researches of M.M. Gruby and O. Delafond. Finally, a short chapter "On the Lymphatic System" closes the division, "Organs of Circulation." From a careful perusal of "The Pathological Anatomy of the Organs of Respiration," we are in a position to state that this division contains a well-arranged description of the results of the researches of a host of investigators. We need not, however, analyze these subjects, as they are familiar to our readers; but we desire to direct particular attention to the views of Dr. W. T. Gairdner† as to the production of emphysema.

* Sydenham edition, vol. iv. p. 165.

† Mémoire sur le ver flaire qui vit dans le sang du chien domestique: Comptes Rendus des Séances de l'Académie des Sciences, tom. xxxiv., Séance du 5 Janvier. 1852.
Monthly Journal of Medical Science, vols. xi. and xii.; also, British and Foreign Medical-Surgical Review, April, 1853, p. 452.

Dr. Jones commences the description of the "Pathological Anatomy of the Alimentary Canal" with an account of the morbid changes that occur in the mouth, fauces, and parts contained in them. The affections to which the teeth are liable are described "from the excellent work of Mr. Tomes on the subject." The "abnormal conditions of the stomach" have been the subject of most interesting original observations, upon which, however, we refrain commenting, as we have seen a notice that Dr. Jones is preparing a work, entitled 'Pathological and Clinical Observations on Diseases of the Stomach.' The diagram of intus-susception will make this change of position readily understood. The results of prolapsus recti are in general much less serious than those of intus-susception; one case, however, in an adult male, came under our immediate observation, in which the prolapsus was as large as the patient's head: he died on the fourth day, before suppuration set in.

Congestion, inflammation, phlebitis, cirrhosis, and fatty degeneration of the liver, the various tumours that occur therein, together with the most important alterations in the biliary passages and in the bile, are well described. The pancreas is, we believe, the least frequently examined of all the viscera of the abdomen, yet its state in diabetes and other diseases is worthy of observation. Dr. Jones treats very briefly of atrophy, inflammation, fatty degeneration, cancer; and to these Förster adds tubercle of the pancreas. So little is known of the pathological changes to which the spleen is liable, that we take this opportunity of mentioning, that in a spleen weighing thirty-one drachms, taken from the body of a child, aged two years, who died of epistaxis, we observed long rhomboidal crystals of ammonio-phosphate of magnesia lying in great numbers in the spleen-pulp. A similar appearance has been noticed by Dr. Jones, and is mentioned at page 581. Further, the occurrence of leucocythemia, that we have observed in two cases of enlargement of the thyroid,* will, if further confirmed than they have already been by Dr. Neale,† be interesting, as affording a connexion between the condition of the blood in splenic and thyroid enlargement.

"The Morbid Anatomy of the Urinary Apparatus" is another division of which Dr. Jones is the author. He considers congenital anomalies, hyperæmia, renal hæmorrhage, anæmia, nephritis, and degenerative disease of the kidney, or desquamative and non-desquamative nephritis, or sub-acute inflammation of the kidney, under each of which names morbus Brightii has been described. The remarks on this latter affection are founded upon the observation of Frerichs, Johnson, and the author, and illustrated with nine woodcuts. This chapter concludes with the anomalous conditions of the urinary passages, bladder, urethra, and urine. The abnormal conditions of the testes and vasa deferentia are described and illustrated after Mr. Curling's article "Testis," in the 'Cyclopædia of Anatomy and Physiology;' these, with the lesions of the prostate and penis, complete the chapter on "The Male Generative Organs."

Dr. Sieveking treats of the "Pathological Anatomy of the Female Organs of Generation," which he divides into four chapters, on the ex-

* Excess of the White Corpuscles of the Blood, occurring in cases of Goitre: Quarterly Journal of Microscopical Science, April, 1853, p. 176.

† Medical Times and Gazette, vol. viii. p. 430, 1854.

ternal and internal organs of generation, the morbid conditions following and preceding parturition, the ovaries and mammae.

We wish to direct the attention of those interested in such subjects to Dr. Heschl's researches (translated by Dr. Robert M'Donnell), which have led him to believe that* "the proper substance of the uterus undergoes so complete a transformation to molecular fat, that not one single fibre of the uterus, existing previous to childbirth, remains behind."

The fifty concluding pages are occupied by the pathological anatomy of the joints by Dr. Jones, and of the bones by Dr. Sieveking. We have already noticed the other divisions of this work at such length, that these important subjects cannot be analyzed.

Förster opens each division of his 'Manual' with a short description of the normal structure of the parts, the abnormal conditions of which he is about to examine; and at the end of each subdivision reference is given to the principal authorities upon the subject. His book is a pretty complete compendium, without containing any original matter. Förster's 'Atlas of Microscopic Pathological Anatomy' consists of eighteen tables, having from twelve to four figures in each; together with eighty-seven pages of letterpress, containing a table of contents arranged, *first*, according to the class of pathological process; *secondly*, according to the organs in which the change occurs; *thirdly*, explanations of each table; and *lastly*, a short description of the figures is given at the foot of each table. These plates are not so valuable as those of Funke's 'Atlas of Physiological Chemistry,' because the latter are drawn as they were observed; while Förster's plates are beautiful pictures and very fine specimens of engraving, but they are too artistic, too nicely filled in, and far too well defined, to be facsimiles of what is seen by the aid of the microscope. It is, however, well suited for showing students, after they have seen an object as it is, what they might see could they make perfect sections; and it must be considered to be the best and most complete atlas of the histo-pathological anatomy of the tissues that has yet appeared.

Drs. Jones and Sieveking's 'Manual' is a more readable and far more original work than its German cousin. In the second edition we hope we shall find a chapter on the method in which autopsies should be conducted: an addition that would be, we presume, most acceptable to students and practitioners. The wood-cuts are, with very few exceptions, all well brought out, and are highly creditable to Mr. Bagg, who, to use the words of the preface, "has achieved as much as could be done by wood-engraving."

In conclusion, those who have followed our review thus far, or perused the original, have, we presume, formed a very favourable estimate of the English 'Manual' as a whole. By studying it, the student will not only learn the most important principles of the science of pathological anatomy, and be thereby materially assisted in appreciating the effects and symptoms of disease, but will be aided in taking a comprehensive view of the phenomena of diseased action. The form of this 'Manual,' as of Mr. Churchill's other publications of the same series, together with its con-

* Researches on the Conduct of the Human Uterus after Delivery. By Dr. R. Heschl. Translated from the German. Dublin, 1858.

densed yet comprehensive information, will doubtless render this work most acceptable to those engaged in the everyday practice of the profession.

Yet a word before we close : the great majority of medical men admit, that pathological anatomy is the base upon which rests rational diagnosis and practice. The teachers of that science have for years past spent both time and labour in their endeavours to create a taste for its study. How long, then, will our universities, colleges, and public boards, allow their students to judge whether it is, or is not, necessary to study the marks left by disease? When will pathological anatomy enter into the curriculum of medical education?

REVIEW VI.

Chloroform, its Properties and Safety in Childbirth. By EDWARD WILLIAM MURPHY, A.M., M.D., Professor of Midwifery, University College, Obstetric Physician to University College Hospital, &c. &c.—London, 1855. pp. 72.

THE increase in the number of deaths during the performance of surgical operations under the influence of chloroform, has naturally excited a good deal of anxiety in the minds of professional persons and of the public, and it has given a colour at least, if not force, to the arguments and assertions of those who have throughout opposed the use of anæsthetics. The mistrust thus excited is doubtless strengthened by the fact, which cannot be denied, that we have arrived at no very satisfactory or positive conclusions as to the cause of death in such cases; whether it has been the result of the mode of administration, as Professor Syme seems to think, or from the presence of some incompatible disease, or some peculiarity of constitution which renders the system intolerant of chloroform—we cannot say, but certain it is that we are not yet in a position to say in what cases it ought to be proscribed. Nay more, it is not certain that, in several of the fatal cases, chloroform had any share in producing the unfortunate result; for many cases are on record of patients who died suddenly at the commencement of an operation, without any adequate cause, long before chloroform was discovered. In these cases it requires more than ordinary skill to guard against the old fallacy of *post hoc ergo propter hoc*. The truth is, that the whole subject requires careful revision; and the object of those who seek for the truth should be, by a careful collation and stringent analysis of facts, and especially of the unsuccessful cases, to ascertain the diseases with which chloroform is incompatible, the states of the system generally in which its administration is injurious or questionable, and to what extent its employment is without risk. If we knew these points, chloroform would rank precisely as any other remedial agent, but until then we shall be liable to a repetition of unexpected fatal accidents.

These considerations apply to its exhibition in obstetric as well as surgical operations, although as yet they have not been forced upon our attention by similar unfortunate circumstances. Yet we cannot deny, *a priori*, that there may be obstetric cases in which so powerful an agent

will be injurious, and therefore it becomes our duty to guard against them by thorough investigation.

Many, however, who do not object to the employment of anæsthetics in obstetric operations, decline to use them beyond these limits, and demand why they should be employed in ordinary labour? To which the advocates of chloroform would probably reply by another question, Why should we not employ them if we find them beneficial? The investigation of these two queries will go far to exhaust the arguments *pro* and *con*, and we propose to lay the matter pretty fully, but as briefly as we can, before our readers.

In this inquiry we shall derive most valuable assistance from the little volume just published by Dr. Murphy, which we have placed at the head of this article. Its tone is so calm and moderate, its facts so clear, its directions as to the mode and extent in which chloroform should be exhibited are so precise, that we think it will quiet the fears which recent occurrences have excited as to the danger to be anticipated from its employment in midwifery, as well as afford adequate guidance to those who are willing to use it provided they incur no danger by so doing.

"Why should you employ anæsthetics in ordinary labour not requiring an operation?" say the opponents, "since by so doing—1, You interfere with a *natural* process; 2, You contravene a special arrangement of Providence; 3, You induce a state of drunkenness, or something equivalent to it; 4, You may suspend uterine action, so as, 5, to involve the necessity of an operation; 6, You may occasion hæmorrhage after delivery, or convulsions, or insanity; or 7, You may cause death."

These are all the objections we remember to have seen against the employment of chloroform in midwifery, and we have endeavoured to state them fully and fairly. Let us now calmly inquire into their validity, merely premising that they assume the exhibition of chloroform in full doses—in other words, the production of complete anæsthesia.

1. It is undoubtedly true, that in giving chloroform in labour we interfere with the natural process, so far as pain is concerned; but so we do when we give laudanum in a painful disease, for pain is as much the natural accompaniment of certain diseases as of labour, although of course labour is not a disease. But this argument comes with a very bad grace from those who would not scruple to induce premature labour, which is a much more positive and decided interference with a natural and healthy process.

2. That the excess of pain, agony, and struggle of labour is the providential punishment of sin, we do not question; but so are labour ("in the sweat of thy brow," &c.) and sickness and death; and if it be wrong to take measures for the relief of the one *because* it was part of a Divine sentence, so, surely it must be equally wrong to alleviate or postpone the other; yet we do not find that those who conscientiously oppose the former have any scruples about the latter. Exactly the same objection was made to vaccination, to steam-boats, and even (in the conscientious tenderness of Mause Headrigg) to winnowing machines. We trust, however, that the very able answers to such objections which have appeared, aided by a little common sense, have had the effect intended, as we have not heard much of late on this subject.

3. A very influential writer on obstetrics, Dr. Ramsbotham, observes, that "those who come forward as the chief advocates for anæsthetics under labour, have entirely denied, or maintained an undisturbed silence respecting its intoxicating properties;" and again:

"But if the case were put fairly and honourably before them (the candidates for chloroform)—if they were informed that they might probably be made *dead drunk*, but must certainly be reduced to that state which the law designates 'drunk and incapable,' how many, it may be asked, of our high-born dames, how many women possessing common feeling, how many indeed, removed above the very lowest orders of society, would be found to avail themselves of the immunity of suffering which anæsthetics hold out, at such a price, at such a sacrifice of moral obligation?"

Very few, we are sure, unless they believed, as we do, that there is not an atom of foundation for Dr. Ramsbotham's statement; and on this subject we confidently appeal to the numerous surgeons and accoucheurs who have witnessed its effects. In answer to Dr. Ramsbotham's assertion "that this state is not sleep, but drunkenness," Dr. Murphy replies:

"I must be equally emphatic, and repeat that the anæsthesia of chloroform has not the least resemblance to drunkenness; they have not a symptom in common. Alcohol mixes intimately with the blood; chloroform does not. The one is highly stimulating; the other not at all so. Alcohol has no anæsthetic power, unless taken in very large quantities, when the imbiber, after a stage of most boisterous excitement, arrives at the condition termed '*dead drunk*,'—the anæsthesia of alcohol. Chloroform manifests this power without the least excitement; it produces anæsthesia, and takes away pain, *without disturbing the intellect in the least degree*. The patient is perfectly herself, and, in the words of Dr. Ramsbotham, can 'expatiate on the relief afforded her.' If chloroform cause sopor, the sleep is perfectly tranquil, the only evidence of excitement (if such should happen) being the occasional mutterings or ramblings in the transition from wakefulness to sleep. The effects of alcoholic potations do not pass away for hours afterwards, because time is required to separate it from the blood. Chloroform is not so dissolved, and therefore rapidly evaporates, saving the patient, in much less than an hour, as much herself as before she inhaled the vapour." (p. 64.)

In fact, whether we note the effect of small or of full doses, whether we observe the initiatory stage, the full effect, or the recovery, the difference from drunkenness is most remarkable, the scientific resemblance, nothing; the subjects are no more legally "drunk and incapable" than they are "drunk and disorderly." That the subjects of a full dose of chloroform or alcohol are insensible, is true; but we might as well assume that travelling by railroad is the same as travelling by coach, because the destination attained by each is the same.

4. In a certain small proportion of cases, chloroform does interfere with uterine action, rendering the pains less powerful, and perhaps less frequent. In Dr. Denham's report* of 56 cases in which chloroform was used in the Rotunda Lying-in Hospital, of which 15 were natural labours, he mentions that in 4 it suspended more or less the natural action of the uterus, which *returned in full force when the chloroform was withdrawn*. This, we think, is a larger proportion than is generally observed, but it does not matter, for common sense tells us that in such cases we

* Dublin Journal, vol. viii. p. 116, new series.

must cease giving chloroform, just as we give up the use of calomel if it cause diarrhoea; it could be no argument against the general use of chloroform, unless this effect were equally general and permanent.

Dr. Murphy thus states his experience:

"The action of the uterus under chloroform is not generally interrupted. The uterine contractions are governed by the reflex or excito-motor and the ganglionic nervous systems. The latter is never influenced, and will always maintain them. The former requires the full dose to disturb its power; a moderate dose (that which I have recommended) has no effect at all on the reflex nerves; nay, it may rather irritate than control their power. The uterine contractions are sometimes increased under the influence of chloroform, and labour makes a rapid progress. It is true this may arise from the removal of the great disturber of uterine action, mental anxiety and dread of pain; but it may also be explained by excitation of the excito-motor nerves, rousing up the uterus to increased action. If, however, the action of the uterus be suspended because these nerves are getting under the influence of chloroform, the effect is only temporary," because the ganglionic system restores the contractions; while the very fact that the reflex system is thus affected, renders the passages much more yielding and dilatable than before." (p. 37.)

5. But this suspension of uterine action, it is said, may not only be complete, but permanent; and Dr. Robert Lee has related five cases in which it became necessary to deliver with the forceps, in consequence of the cessation of pains, and in which chloroform had been given, although we are not told to what extent, in each case. Now, it will be recollected that in the cases we have quoted from Dr. Denham, the pains immediately returned on the omission of the chloroform; this is the experience, too, of Drs. Simpson, Beatty, Murphy, and McClintock, and certainly our own. We have never seen any exception to this rule, but we have seen cases of powerless labour without chloroform, in which the pains gradually ceased, and the forceps became necessary, and so, we doubt not, has Dr. Lee. To establish his point, he should give us presumptive proof that the cases would have terminated naturally but for chloroform, or some evidence more special of the direct evil effects of the vapour. Upon the data before us we must confess that we must decline agreeing with his conclusions, although we cannot but say that if the practitioners who had charge of the cases thought that the chloroform diminished the pains, they acted very injudiciously in not instantly suspending it.

Dr. Lee relates two other cases in which craniotomy was necessary, as he considers, in consequence of the suspension of the pains by chloroform. In case 1, where insensibility was not produced, it does not appear that the pains were suspended; in the other they were, but whether by chloroform or not is not clear. Dr. Beatty's remarks upon these cases are so apposite, that we shall take the liberty of quoting them:

"We all know that uterine contractions are often suspended naturally for hours in the middle of a labour, when no chloroform has been used, but that alone would never lead one to resort to craniotomy; there must be something else in the case besides mere want of action in the uterus to warrant such a proceeding; and so it must have been in these cases cited by Dr. Lee. Very likely the uterine action was interfered with by a precipitate employment of the drug; but it is also likely that the cases were such as would have required craniotomy equally if chloroform had never been used; for I cannot for a moment imagine that a physician of Dr. Lee's experience would resort to such an operation on the simple grounds of an

arrest of uterine action. In the absence of details of these cases, we may safely put them down as *post hoc ergo propter hoc* cases, and class them among the absurd exaggerations (to use no more severe term) with which partisans so often attempt to mislead their readers."*

Let us also just remark, that the arrest of pains took place during the first stage, a time when their spontaneous diminution in pregnancy is not uncommon and is of no consequence, if other circumstances are favourable; and during which stage, in the absence of complications, an operation is rarely, if ever, justifiable.

6. Dr. Lee relates seven cases in which "insanity and great disturbance of the functions of the brain followed its use;" or, in obstetric language, some degree of puerperal mania occurred; and if this never happened except when chloroform had been used, they would exactly prove the point, but as all the best writers on obstetrics, before and since chloroform, have described such an affection of childbed, and as all of us are unfortunately familiar with it in practice, it is difficult to see how such a case as the following proves anything but that puerperal mania may occur after chloroform, as well as without it. "Case 10. In the month of June last, chloroform was cautiously administered to a lady in her first confinement, twelve hours after its commencement. The pains were soon wholly suspended, and it became necessary to deliver with the forceps. Eight days after, violent cerebral disturbance ensued, and she has continued, till a very recent period, insane." Now, considering that in this case there are two very serious deviations from the all but universal action of chloroform, we think that it rests with Dr. Lee to prove that the necessity of the forceps and the occurrence of insanity were due to chloroform; might we not as well attribute the insanity to Dr. Lee's use of the forceps, and on exactly the same process of reasoning?

"I have administered," says Dr. Murphy, "chloroform in upwards of a hundred instances, a number sufficiently large to form an accurate opinion on such a point. I can truly say that in not a single case was there the slightest approach to mental aberration. Could this happen if insanity were one of its effects?" And again, "I have not as yet met with an instance of mania *after* the administration of chloroform, although it is quite possible I may do so, when the usual causes of this disease come into operation to produce it. I once attended a lady (the wife of a medical friend) to whom had I given chloroform I should have been very much blamed. She suffered rather severely. I proposed the inhalation of this vapour; my friend however objected, as he feared, from some peculiarity in her constitution, that it would not agree with her. She went through her labour, and after a severe trial was safely delivered. On the tenth day symptoms of mental aberration manifested themselves, which continued some time before they disappeared." (p. 67.)

The experience of most of those who have used anesthetics, Drs. Simpson, Channing, Beatty, Denham, &c., quite supports Dr. Murphy's statement, and would almost, if not quite, justify the conclusion that puerperal mania is rather less common when chloroform is used. Although a single fact is not of much value, we may mention that in a case under our own care, in which mania and insomnia occurred without chloroform, the only rest or relief the patient obtained was after the full exhibition of the vapour.

Again, we are told by the same author that "dangerous or fatal peritonitis or phlebitis ensued after the exhibition of chloroform in cases 7, 8, 11, and 13." But here there is the same absence of any attempt to show the connexion between the chloroform and its supposed consequence. Take, for example, the following case:

"CASE 11.—About the same time, chloroform was exhibited to another patient, a lady in her first labour. The contractile powers of the uterus were soon wholly suspended, and the delivery was completed by the forceps. Fatal peritonitis, with peculiar nervous symptoms, soon supervened."

Now, unless we are prepared to take Dr. Lee's word for it, we do not see on what ground we are to attribute the peritonitis to the chloroform; if on the doctrine of sequences, then the operation with the forceps comes after the chloroform, and has consequently a clearer right to be considered its cause.

It is proverbially difficult to prove a negative, and, therefore, without denying the *possibility* of grave accidents from chloroform given in labour, we shall be quite contented, if our readers are satisfied, from the foregoing considerations, that these facts upon which Dr. Lee founds his opinions are "not proven" to be liable to the interpretation put upon them.

But with regard to another effect attributed by him to chloroform, we have more positive evidence in answer. Case 14 is given as one of epilepsy or convulsions caused by chloroform; and we remember the time when it was thought that in certain cases chloroform might produce such an effect, but the result of further experience has been the discovery that, in many cases, the inhalation of chloroform or ether is one of the most powerful means we possess for controlling and curing convulsions. For example, Dr. Channing, of Boston, U.S., gave ether in 10 cases; in 6, the patients recovered;* Mr. Turner,† Mr. Norris,‡ and Dr. Keith§ each gave it in a case with perfect success, and an equally favourable example occurred at Gosport.|| Mr. Bolton tried it successfully after bleeding and opium had failed.¶ Dr. Shekleton, the late master of the Dublin Lying-in Hospital, administered it in 9 cases; in 5 the convulsions were completely arrested, and in 4 they were lessened in intensity and frequency. We have ourselves given it in convulsions during gestation, with immediate benefit. Many more similar cases might be adduced, but these are surely enough to oppose to the single case related by Dr. Lee, and more than sufficient to exonerate the chloroform from having caused the convulsion, unless we admit (on the principle of *similia similibus curantur*), that it may be at once the cause and cure of the disease.

Lastly, a fear was expressed by some practitioners that, as chloroform sometimes interferes with uterine action, it might give rise to hæmorrhage by arresting contraction after delivery; but such has not been found to be the case. We are not aware that any instance of the kind is on record; and we have the testimony of Dr. Simpson and others that it has been safely administered in cases of placenta prævia and other forms of alarming hæmorrhage.

7. We do not know of any case of labour, supposed to have terminated

* On Etherization in Midwifery, pp. 307, 330.

† Edinburgh Monthly Journal, May, 1849.

‡ Medical Times, March 23, 1850.

§ Lancet, Jan. 12, 1850.

¶ Ibid., Aug. 1850.

¶ Lancet, Jan. 29, 1852.

tally from chloroform, related on good authority, except the one given by Dr. Ramabotham, the substance of which we shall quote, together with Dr. Murphy's comments, with which we entirely agree.

"It was the lady's fourth child; she gave birth to the first, after a very tedious and painful labour, in consequence of a considerable narrowing in the conjugate diameter of the pelvic cavity, which rendered the use of the long forceps necessary. This was before the application of anæsthetic agents to the practice of obstetrics was adopted in England. Her medical attendant (not Dr. R.) at her urgent request placed her under the influence of chloroform during her second labour, and her recovery was speedy and perfect."

It was exhibited in her third confinement, but inefficiently, and in her fourth labour,

"Parturient pains came on about noon. Chloroform was given at 7½ P.M., when the os uteri had acquired the diameter of an orange, and the pains had become frequent and strong. Its effects were most delightful and tranquillizing. After refreshing sleep, she rose and bore some moderately strong pains without a return to chloroform. It was then resumed, and repeated in frequent drachm and half-drachm doses, but only when she entreated to have some of the delightful chloroform, from about ten to a quarter to twelve, soon after which the child was born. She instantly expressed much gratitude, and expatiated on the relief afforded, though even then she felt 'wring' by the severity of her labour. The uterus contracted well, and the patient appeared comfortable. At the end of an hour and a half, however, distressing dyspnœa came on; this was soon followed by convulsions and almost immediate death."

"Such," observes Dr. Murphy, "is the account of this deplorable death from chloroform. Let it be compared with those which have occurred in surgical practice, and a judgment formed of the accuracy of this conclusion. The lady referred to never lost her consciousness. Chloroform never caused her the slightest inconvenience during its inhalation, her respiration was perfectly undisturbed. After delivery she *instantly expatiated on the relief afforded her, was quite collected, quite comfortable.*" But in an hour and a half afterwards, when all the chloroform inhaled (and she had evidently taken very little) had quite time to evaporate and disappear, she is seized with a distressing dyspnœa! an effect the very contrary to what chloroform is known to produce; *convulsions and death follow.* We would ask, how could the absent vapour cause a distressing dyspnœa, when its presence did not disturb the respiration in the least degree? If chloroform cause distressing dyspnœa, why not do so during its inhalation? or if such be its effect *after* inhalation, how is it that we cannot find a single instance of death so produced in the thousands who have been under its influence for surgical operations? A moment's reflection is sufficient to show the injustice of attributing this fatality to chloroform, simply because no other explanation can be given," (p. 61.)

To us this case resembles those cases of idiopathic asphyxia which occur sometimes after delivery, and of which we had intended to quote one or two as illustrations, had not this notice already occupied so much space.

In addition to the foregoing serious objections to the use of chloroform, a minor class has sometimes been adduced, consisting of certain symptoms which occasionally follow its employment. For example, headache, vomiting, and temporary incoherence, do occasionally but rarely occur: the former speedily pass away, and the latter is removed by a little less or more of the vapour; we have never observed that the language was either violent or coarse; in most cases it was unintelligible, from the patient uttering only parts of words. As to erotic ideas, said to be excited by

chloroform, it is difficult to speak positively; 'at any rate, it cannot be a common effect, as few with whom we have spoken have witnessed it.

We have thus endeavoured to lay before our readers all the objections brought against the use of chloroform in midwifery, and we have endeavoured to do so fairly, neither extenuating nor undervaluing the arguments of its opponents, nor denying the possibility of accidents. But we must here remark a very important point: all, or almost all, the objections are directed against the use of chloroform in full doses—i.e., given so as to produce insensibility or sleep; and although we think the facts adduced fail to establish the case even against its full exhibition, we have ourselves an objection to carrying it to this extent in ordinary labour, for this very obvious reason—that it is *quite unnecessary*. Quite sufficient relief may be afforded without interfering with the mental condition of the patient—such relief as will enable her to bear her labour firmly and patiently, and secure her from all the evil consequences of prolonged pain. This, we say, may be done without placing the patient within the possibility of danger; and to this, therefore, none of the objections hitherto made apply. Dr. Murphy's testimony is conclusive upon this point:

"The obvious conclusion," he remarks, "from these experiments is, that the risk from chloroform may be altogether avoided, and yet the patient receive a considerable amount of relief. In the practice of midwifery, the pains of labour can be assuaged and rendered tolerable without inducing sleep; and in the practice of surgery, it appears to me that many minor operations may be performed with equal safety." (p. 52.)

On the other side of the question, the advocates of chloroform contend for its exhibition, because—1. You relieve suffering by it; 2. You diminish the nervous irritation produced by long-continued pain; 3. You lessen, or avoid altogether, the shock to the nervous system, and so far the object may be obtained without producing sleep or unconsciousness; 4. In case of an operation, the patient is not merely saved from suffering, but placed in a more favourable condition for its skilful performance, inasmuch as the operator has not to guard against her struggles and resistance where chloroform is fully administered.

1. That pain is an evil can hardly be denied, but that a certain amount of it can be borne with perfect impunity is equally true: we daily witness labours terminated within a few hours without any perceptible effects upon the constitution, or any retardation of the recovery. In such cases, we are far from thinking chloroform necessary, but if the patient request to be spared a portion of this amount, and we believe that we can do so with perfect safety, why should we refuse? Our own practice has been, never to propose chloroform unless we saw that the amount of suffering was likely to do mischief, or in cases where an operation has been necessary; but we have not felt at liberty to refuse it even in less severe cases, when demanded, and we believed that its administration was safe.

2. But in many cases the suffering is very severe, either from the exquisite sensibility of the patient, or from the greater resistance; and, no doubt, the recovery may be retarded by it. From this injury we possess the means of saving our patient, in the moderate exhibition of chloroform.

"The advantages of chloroform in obstetric practice consist not only in its power of controlling the intensity of suffering to which the parturient woman is

too often unnecessarily exposed, but in promoting a more favourable recovery. Since the publication of Mr. Travers' work 'On Constitutional Irritation,' the profession acknowledge the danger that sometimes results from intense pain. Patients have died from the shock of an operation. It is denied, however, that the pains of labour, be they ever so intense, produce any shock to the constitution: I believe this to be utterly untrue. I know nothing that predisposes more to troublesome consequences than long-continued and severe pain, especially with delicate women. Their recovery is always slow; and while in this depressed state, if a morbid poison be within reach, they are sure to absorb it." (p. 44.)

3. But the shock to the nervous system may not only retard the patient's recovery, but may implicate her in immediate peril. We have more than once witnessed cases of labour terminated by the natural powers, and yet which left the patient in such a state from the "nervous shock," that it was doubtful for some time whether she would ever rally; and in one such case, death took place apparently from no other cause. Again, in cases not so severe, but in which an operation may be necessary, this addition to the shock may leave the patient with a very doubtful chance of recovery. From these dangers we may, in most cases, preserve our patient by a timely and moderate exhibition of chloroform, without incurring any risk of injury.

4. Lastly. The situation of a patient upon whom an obstetric operation has to be performed is very different from that of a surgical patient, and renders the benefit of chloroform still more striking. The latter has a choice offered him, and if he submit to the operation he does so willingly, and with an effort at least to bear it bravely. Besides, there are always assistants at hand who will exercise both control and even a little coercion, if necessary, after the operation has been commenced. On the contrary, an obstetric operation is a comparatively sudden necessity, without a choice whether the patient will submit or not. She must consent to its performance to save her own life, or her child's, or both, and this urgency, together with the effect of her previous sufferings, seems to diminish her power of self-control. Then, if during the operation she resist or struggle, there is no power of restraining her, nor would it be easy, or perhaps safe, to do so, even if there were plenty of assistants at hand. Yet, notwithstanding the outcries and struggles of the patient, an important operation, internal, involving organs essential to life, and easily injured, in the neighbourhood of some of the great viscera of the body, has to be performed coolly, discriminatingly, and deliberately, under most trying circumstances of fatigue of body and mind, and possibly with many other disadvantages. How difficult this is, even in favourable circumstances, all obstetricians know full well, and those in contact with the poor know how almost impossible it occasionally is. But under the effects of a full dose of chloroform all this distress is spared; the patient, unconscious of suffering, lies sleeping calmly, perhaps smiling, while the operator, relieved in mind and body by the absence of cries and struggles, is at liberty to concentrate his entire efforts to the successful completion of the formidable operation he has undertaken; and when all is finished, and the effect of the chloroform dissipates, he finds his patient awake, calm, easy, and grateful for the relief he has afforded and the suffering she has been spared. That this is no exaggerated statement will be testified by every one who has fairly tried the experiment.

Until we have better evidence than has hitherto been adduced, that the full use of chloroform in such cases is attended with danger, we confess that we should feel ourselves blameworthy if we refused to employ it. Judiciously given, it is a most valuable addition to our means for rendering an operation successful, both by relieving the patient's suffering and rendering her a better subject for the skill of the operator. To those who think that they derive any benefit or guidance from the outcry of the patient, we hardly know what to say without giving offence. The true guide for an operator ought to be his perfect knowledge of the organs upon or among which he is going to operate; and if this be insufficient, without the addition of warning from the cry of his patient, it would perhaps be better that he should not operate at all.

We have occupied so much space in these investigations, that we shall only lay before our readers Dr. Murphy's rules for the administration of chloroform, premising that we ourselves prefer a white pocket handkerchief, folded in a conical shape, to any inhaler we have hitherto tried, and that we think the dose rather to be measured by the sensibility of the patient, and the effects produced, than by minims or drachms.

"Rule 1. Let the chloroform be pure. If rubbed on the hands, the smell should be fragrant, not pungent, like sulphuric ether. If inspired from the inhaler, there is a sense of warmth in the mouth; a fruity flavour, no pungency; if the strength of the vapour be sufficient, it will excite a slight cough: but if impure, the cough is irritating. Let the sponge of the inhaler be placed in warm water, and then wrung perfectly dry. About thirty minims may be poured upon it, which is sufficient in the first instance.

"2. When labour has commenced, do not interfere so long as the patient bears her pains well; if she be not teased with short, very severe, and inefficient pains, chloroform need not be given. If, on the contrary, the severity of the first stage be such, the anguish of the patient so great, that pain is evidently a cause of protraction, chloroform may be given with great benefit.

"3. Always commence with a small dose, about thirty minims; if it agree with the patient no inconvenience is caused, but she will generally complain that it is doing no good; the quantity may then be increased, until on inhalation the exhibitor finds that she cannot take a full inspiration without cough.

"4. In the second stage of labour, chloroform may be given when the head is approaching the perineum, or before then if the pains become intolerable. This may be known not merely by then greater intensity while the uterus is in action, but also by the restlessness of the patient in the intervals. She is watchful, dispirited, still crying, but in a more subdued tone, from pain and a feeling of soreness.

"5. When the head arrives at the perineum, chloroform may be given in a fuller dose, if it have not already accumulated. The perineum yields more easily under its influence, and the severity of the pains is controlled without any loss of force. This rule applies especially to cases in which powerful forcing pains are acting against the perineum at the hazard of its laceration.

"6. When operations are necessary, if they are not severe—as, for instance, some forceps operations—chloroform may be given in the same manner as in natural labour, but always after the instrument is applied.

"If severe, it may be given as in surgical operations, but not to the same extent. Hence an assistant is necessary who is conversant with the properties of this anæsthetic. It is obvious that the same person cannot operate and give simultaneously the full soporific dose of this agent.

"7. The inhaler should be applied to the mouth just before the pain commences, two or three full inspirations taken, and the moment the action of the uterus

ceases it should be withdrawn. The inhaler should never be applied in the interval between the pains, and if used in the middle of a pain the cries of the patient blow away the vapour, and no relief is given.

"8. When inhalation has been continued in this interrupted manner for some time, if any alteration be observed in the countenance or manner of the patient—if the face is flushed, or bloated, or tinged with a slight lividity—if she ramble or become hysterical, let the inhaler be withdrawn, and the face of the patient fanned. Wait until the pains return to their original severity before renewing the inhalation, when it is probable that these symptoms will not return.

"9. In some instances, the patient is very intolerant of her pains, and if given chloroform to relieve them, she becomes hysterical, crying, perhaps, louder than before it was inhaled. In these cases, it is better to induce sopor, which may easily be done, without stertor. For this purpose, a sponge and folded handkerchief applied to the nostrils is preferable to the inhaler. Whenever sopor is brought on, the closest attention should be given to the countenance—observe the irritability of the eyelids; to the respiration—notice its frequency, and especially stertor; to the pulse—mark its strength. The handkerchief should always be held at a distance at first, and be gradually brought nearer, but the sponge should never be applied quite close to the nostrils.

"10. There should be the freest circulation of air in the apartment; and if, after delivery, there should be any feeling of faintness or nausea, ammonia in effervescence will relieve it." (p. 69.)

We strongly recommend Dr. Murphy's little work to the profession, which we think owes him obligation for having brought clearly before it the advantages of chloroform in small doses for the relief of the suffering of labour.

Fleetwood Churchill.

REVIEW VII.

1. *The Decline of Life in Health and Disease; being an Attempt to Estimate the Causes of Longevity.* By B. VAN OVEN, M.D.—London, 1853. pp. 300.

2. *De la Longévité Humaine, et de la Quantité de Vie sur le Globe.* Par P. FLOURENS.

On Human Longevity and the Amount of Life on the Globe. By M. P. FLOURENS.—Paris, 1855. Small 8vo, pp. 240.

M. FLOURENS treats of certain topics which have reference to the theory of life. In every age, the author observes, this has been an object of study, while it is only now that we have begun to study it in its largest aspects. The questions of the quantity of life, and of the first appearance of life upon the globe; of the permanency of species, and of lost or extinct species—are, M. Flourens remarks, entirely new. Along with these, the author discusses other more ancient questions, but which he thinks he has invested with some degree of freshness; these are—human longevity, the formation of life, and old age. M. Flourens' complete 'Theory of Life' has yet to appear.

In the first part of his work—that on 'Human Longevity'—M. Flourens brings forward the example of Louis Cornaro, who, by dint of extreme moderation and sobriety, prolonged his life to the length of nearly a century. The age of one hundred years is regarded by M. Flourens, on

the following physiological grounds, to be the period of human longevity. Taking the period of increase or growth of the human frame—that, namely, up to the age of twenty—by which time the epiphyses of the long bones are found to be firmly united to their shafts, and multiplying this age by five; the period of one hundred years is obtained. It must not, however, be forgotten that M. Quetelet and others have fixed upon twenty-five years as the period of increase; and that very different multipliers and multiplicands have also been given by other physiologists. M. Flourens supports and illustrates his theory by an examination of the various stages of existence or periods of growth, maturity, and decline in man and in the lower animals.

"The duration of life," observes Buffon, "is not regulated by climate, food, manners, or external condition, but by fixed laws of the animal economy." This opinion is based upon correct observation. We observe that each species has its distinctive characters; however nearly they may resemble in some points, there are features belonging to each sufficiently different to establish their distinctness.

Each species, remarks M. Flourens, has its determinate duration of gestation and of growth, as well as its determinate size, figure, &c.; wherefore, then, he asks, should there not be a fixed law of duration of life in each species? Buffon, accordingly, thought he saw the operation of this law in the multiplication by seven of the period of growth, which he fixed at the age of fourteen. This gives about the same duration of life as is deduced from the more philosophical and exact observations of M. Flourens.

There are countless instances of longevity on record in which persons have been said to have lived beyond a hundred years. We admit that the truth of many of these is placed beyond doubt or dispute; therefore, without incurring the charge of scepticism, we may observe that these instances are met with before accurate registration was enforced. Future generations will, however, have in their possession the means of verifying the fact in every alleged instance of longevity.

The comparative observations of M. Flourens lend confirmation to his law as regards the age of man. Thus the camel has attained its growth by the age of eight years; the horse, at five; the ox, at four; the dog, at two; the cat, by eighteen months; the rabbit, at twelve months; the guinea-pig, at seven months; &c., &c. Multiplying these figures by five, we have the usual length of life in these animals, e. g., of the camel, forty years; of the horse, twenty-five years; of the ox, from fifteen to twenty years; of the lion, about twenty years; of the dog, from ten to twelve years; of the cat, from nine to ten years; of the rabbit, about eight years; of the guinea-pig, from six to seven years; &c., &c. The theory is thus found in accordance with facts. In most other animals the data for the calculation are yet wanting.

M. Flourens points out a relation between the principal phenomena of animal life; thus, the duration of life is measured by the period of growth; the period of growth has also given a relation to that of gestation; the larger the animal, the longer the period of gestation; the latter, in the rabbit occupies thirty days, in women nine months, in the elephant nearly two years.

Of the duration of life in the elephant, observes the author, we are ignorant. If, however, the author's theory be applicable in this instance,

the age of an elephant may be as great as is commonly received; thus, 9 months (the period of human gestation) $\times 25 = 20$ years; (the period of growth) $\times 5 = 100$ years (the extreme duration of life in man). Applying the same calculation in the case of the elephant, e. g., "nearly two years," (say 22 months) $\times 25 =$ nearly 46 years (the period of growth of the elephant) $\times 5 = 230$ years, we have the duration of life in the elephant. Observations are wanting for the verification of this calculation, but the result does not differ widely from recorded instances of elephantine longevity.

M. Flourens cites from various authors numerous instances of extraordinary longevity in men and lower animals. Dr. Van Oven has given tables comprising the names, condition, country, date of death, and ages of fifteen hundred and nineteen persons who have attained to ages between a hundred and a hundred and ten years; of three hundred and thirty-one who died between the last-named age and one hundred and twenty years; of ninety-nine who reached the age of a hundred and thirty; of thirty-seven who lived to be a hundred and forty years old; of eleven who reached a hundred and fifty; and of seventeen who exceeded the last-named age. Besides these, Dr. Van Oven has collected notices of fifty living persons at ages varying from one hundred to one hundred and eighty years! Nor are these all that Dr. Van Oven has brought together. Four hundred and ninety "additional instances" of longevity, two thousand one hundred and seventy-nine instances of ages above one hundred in Russia, seven hundred and fifty ditto in Sweden, and the quotation from the Registrar General's Reports of seven hundred and eight deaths above one hundred years of age in England and Wales, in the five years 1838 to 1844. In all, seven thousand who have lived from a hundred to a hundred and eighty-five years.

Dr. Van Oven does not vouch for the genuineness and authenticity of all these records; but they may be received, in the words of the author, "as more than enough to justify a fair presumption that human life might endure much longer than it usually does, and to encourage the exertions of those who desire to promote healthful longevity."

Dr. Van Oven's work has a chapter upon the Causes of Longevity, which he finds in the original constitution of the individual, his habits, avocation, mode of life, immunity from disease, equanimity of temper, and freedom from great and frequent excitement. The attaining that (to some persons) very desirable point, an old age, is in consequence resolved, in the work of Dr. Van Oven, into the regulation and care of all these conditions. The same author, moreover, devotes a dozen chapters to "Diseases of Age," and to "The Decline of Life in Disease;" having introduced the study of longevity by several chapters "On the Process of the Organization of Man, from Birth to Maturity."

M. Flourens having, as above described, expounded and illustrated his theory of the duration of life, proceeds to discuss certain subjects which he regards as collateral to the question of human longevity and his theory of life. The topics to which we refer, are the permanency and extinction of species, spontaneous generation, fossil remains, the deluge, &c. M. Flourens has treated all these topics in an interesting and lucid manner, but he has not therein presented us with any facts that are not to be met with in standard works on zoology, geology, and natural history in general.

REVIEW VIII.

1. *Die Hämodynamik nach Versuchen.* A. W. VOLKMANN. (Cap. xii. *Von den Kräften welche das Blut Bewegen*).—Liepzig, 1850.
On the Forces which Move the Blood. By A. W. VOLKMANN.
2. *Le Système Capillaire Sanguin.* Thèse par L. A. SEGOND.—Paris, 1853.
On the Sanguineous Capillary System. By L. A. SEGOND.
3. *Principles of Comparative Physiology.* By W. B. CARPENTER, M.D., F.R.S., F.G.S. (Chap. v.)—London, 1854.

It is now more than a century since some few physiologists were led to suspect, from the observation of certain phenomena which they could not otherwise explain, that during the passage of the blood through the capillary vessels it was subjected to a force, independent of the action of the heart, which assisted the circulation. Various conjectures were then formed of the nature of this power, and its source was generally supposed to be either in the blood itself, or in the walls of the capillaries.

But the doctrine of a force resident in the capillaries, supplementary to the heart's action, in circulating the blood, was, by the great majority of physiologists, rejected. They saw no evidence of its existence, and, moreover, they calculated that the powers already known, and perhaps understood, were amply sufficient for the purpose, and hence they negatived the idea of a supplementary one. This question has since been repeatedly discussed, and many excellent names may be appealed to in support of either view. The rapid advances which physiology has made of late years, have greatly tended to dispel the mystery in which this subject was formerly enveloped; but the question still continues to receive opposing answers, and in the most recent authors and most eminent physiologists of the present day, advocates of either doctrine may be found. Moreover, those who admit that the blood in its passage through the capillaries is subjected to a force which influences its circulation, are by no means agreed as to the extent of its operation. Here the greatest diversity of opinion prevails, from that of Bichat,* who imagined that the influence of the heart did not extend beyond the capillaries, to that of Dr. Allen Thompson and others, who believe that the power which operates on the blood in the capillaries is only capable of modifying its distribution—not contributing to its progressive motion.

Many arguments which were formerly advanced in relation to this subject have been subsequently either altogether rejected or considerably modified, while new ones have been supplied as physiology in its progress has swept away former errors, or revealed new truths. The question is an important one, and it will be interesting to inquire into the nature and extent of the evidence concerning it which we at present possess.

The facts which relate to this inquiry are both numerous and various.

* See also Guthrie, who says, "The heart exerts a comparatively trifling degree of influence on the circulation." *On the Diseases and Injuries of Arteries, &c.*, by G. J. Guthrie, p. 226, 1850.

These we shall attempt to arrange into more general expressions, as arguments either for or against the doctrine.

In support of the doctrine:—

1. The circulation of the nutritive fluid is naturally maintained, in various instances, independently of a heart, by the operation of other causes.

2. After the heart has been removed, or after the great vessels at their origin have been tied, the circulation is continued for some time through the capillaries.

3. The empty state of the arteries, so generally seen after death, is not altogether due to the contraction of their walls, but is partly dependent on the continuance of the capillary circulation.

4. In its healthy and natural circulation the blood is subjected to continual changes during its passage through the capillaries, both in its velocity and direction, which cannot be attributed to any influence derived from the heart or vessels.

5. The amount of blood circulating in certain parts or organs is liable to considerable variation, independently of any general change.

6. The quantity of blood flowing through the various arteries is regulated by causes operating in the parts which they supply.

7. The circulation of blood through any part or organ is in a great measure dependent on the normal changes which it undergoes in the capillaries.

Against the doctrine:—

1. It is alleged that some of the preceding statements will not stand the test of a rigid investigation, and that the phenomena actually observed may be otherwise explained.

2. A force not exceeding the natural action of the heart has been proved by experiment to be sufficient to propel the blood through the entire system.

3. When the circulation of a limb is confined to the branches of a single artery and corresponding vein, the flow of blood through the latter is directly controlled by pressure on the former.

These several statements will be now separately considered.

1. In plants, the sap circulates independently of any contractile or propelling organ; and it has been proved by well-known experiments (see those of Hales and others) that in the higher plants, at least, there are two distinct forces in operation—one at the roots (*vis à tergo*), and the other at the leaves (*vis à fronte*)—and that these forces operate so long as the vital changes continue, and are dependent on them.

But the best illustration is to be found in the circulation of “the elaborated sap or latex, which from its containing the elements for the nutrition, and for the various secretions of the plant, may be likened to the arterial blood of animals.” (Todd and Bowman.) This nutritive fluid circulates through a distinct system of anastomosing vessels, the laticiferous, closely resembling the capillaries of animals, in the leaves and bark. The fluid moves in various directions, even in contiguous vessels, with a velocity by no means uniform. The circulation is most vigorous in rapidly-growing parts. Its rate seems to depend upon the activity of the nutritive process. It is often observed to circulate in a

direction opposed to the force of gravitation, as towards the stem in a dependent branch.

In some of the lower animals a distinct circulation may be observed, and yet no special organs for the propulsion of the blood can be detected; and the heart in its simplest condition, as the dorsal vessel of insects, is manifestly incompetent, if unassisted, to maintain the circulation.

In the higher animals, the lymph and chyle are propelled onwards in their vessels with considerable force—a force which can be in no way dependent on the action of the heart.

In the early embryo of the higher animals a distinct movement of blood has been observed in the vascular area before any pulsating organ could be detected. Moreover, it has been affirmed by very excellent observers, that the first motion of the blood is *towards*, not *from*, the centre.* These points, however, have not yet been determined beyond all doubt.

In reply, it may be argued that the above facts, although true in themselves, yet have no direct bearing upon the question at issue. Because, in plants and in the lower animals, and even in the earlier conditions of the higher, causes independent of the action of a heart may act to circulate the blood, it does not follow that in the adult of the higher animals such causes shall still continue in operation. In short, it is denied that this kind of evidence is admissible.

In answer to these objections it may be replied, that although the above facts are not brought forward to prove the doctrine, but merely to show that other causes than the action of a heart may circulate the blood, yet it does not follow that because these causes, which operate independently of a heart in plants and the lower animals, are subordinate to the action of a powerful heart in the higher, that therefore they no longer exist. Such an idea is opposed to one of the most interesting facts displayed by the study of the organic kingdoms. It is well known that in tracing any particular structure or function, from the lowest to the highest living beings, through the regular series of organized forms, or even through the successive stages of development of the higher, we are led to perceive a great principle of progression from a more general to a more special character. For example, the function of respiration. In the simplest forms of animal structure (Polypes, Medusæ, &c.) no special organ exists for this purpose: in them, as in plants, the aëration of the fluid is accomplished at the surface of the body, simply by exposure to the surrounding medium, through the membrane which forms their integument. As we ascend the series we find that the external membrane is extended inwards into cavities among the viscera, or outwards from the surface of the body; and that in these organs, more particularly, the necessary changes are effected in the blood. Thus the rudiments of lungs or branchiæ appear. These organs, which at first present themselves as simple prolongations of the external membrane, gradually assume more

* Von Baer: Ueber Entwicklungsgeschichte der Thiere, &c. With regard to the development of bloodvessels in effused lymph, Mr. Paget says: "Although direct observations are wanting, I think we may conclude that all the vessels of inflammatory lymph are formed by outgrowth from adjacent vessels, as in the process of repair, and that through these vessels, not by its own development, it derives its supply of blood."—Lectures on Surgical Pathology 1855, vol. i. pp. 367—8.

complex forms, until, when we arrive at the higher animals, we find special complicated organs adapted for the aëration of the blood. But as it is thus seen that "a special function arises out of one more general, and this by a gradual change,"* the existence of another no less interesting fact is also to be observed. It appears that even in the highest animals this limitation of function to a special organ seldom proceeds so far as to completely exclude or obliterate, however it may supersede or obscure, its more general character. "The function of respiration, is not confined to the lungs, even in animals which possess them in their most developed form. The blood which circulates through the capillaries of the skin is aërated by communication with the atmosphere, wherever there is no impediment offered by the density of the tegumentary covering." And this is true even of the human body. This law has been thus stated, "In cases where the different functions are highly specialized, the general structure retains, more or less, the primitive community of function which originally characterized it."

Now the circulation affords another example, equally evident, of the same general laws. Although in the higher animals we have this function principally discharged by a special central organ, it by no means follows that the more diffused force, which is alone employed in the lower animals, is here annulled, although subordinate to, and obscured by, a far more effective power. On the contrary, we should reasonably expect to find distinct traces of it still existing. At all events, it will be admitted that these facts clearly prove that the movement of the blood can be accomplished by other causes than the action of a heart.

2. The circulation of the blood in the capillaries, after the influence of the heart's action has been removed, has been repeatedly witnessed. Dr. Wilson Philip relates the following experiment:

"A ligature was thrown round the vessels attached to the heart of a frog, and the heart was then cut out. On bringing the web of one of the hind legs before the microscope, the circulation in it was found to be vigorous, and continued so for many minutes; at length gradually becoming more languid."†

This experiment has been repeatedly confirmed.

But in the hands of other observers this experiment has not been attended with such decisive results. Thus, Dr. M. Hall relates the following:

"A ligature was applied round the aorta of a frog: the circulation in the web, which was previously very vigorous, was almost immediately arrested, first in the capillaries, then in the veins. In the arteries there was a singular oscillatory motion of the blood for ten or fifteen minutes. The globules of the blood proceeded slowly onward for some seconds; there was then, all at once, a rapid retrograde movement of the blood, apparently through the same space. This oscillation was repeated; the globules of the blood were again moved alternately in progressive and retrograde directions as before."‡

A similar description has also been given by other observers. Haller

* On Unity of Function in Organized Beings, by W. B. Carpenter: Edinburgh New Philosophical Journal, 1837. See also Von Baer: *Über Entwicklungsgeschichte der Thiere*. Königsberg, 1826—37.

† An Experimental Inquiry into the Laws of the Vital Functions, by A. P. W. Philip, p. 21. 1826.

‡ An Essay on the Circulation of the Blood, by M. Hall, 1831, p. 78.

appears to have performed many of these experiments, but not with a very uniform result. In several, the phenomena observed after excision of the heart, or section of the great vessels, varied. In many, the blood continued to move in its natural direction; in some instances it became oscillatory, and in others retrograde. In many of these experiments he states that "after the heart had been removed, or a ligature had been applied to the aorta, or after its two great branches had been divided, the motion of the blood in the arteries continued, even for a considerable period; and he says, "It hath seemed to me, that the motion is better preserved in the veins than in the arteries."* Indeed, he observed that after division of the vessels near the heart, the blood moved onward longer in the veins than in the arteries; it continued to move in the veins after the arteries were emptied.†

It appears to us that the majority of these experiments, which were attended with very decided results, ought to outweigh in value the remainder: for when such experiments are frequently repeated, it is not surprising that the results should sometimes vary, for they are amenable to the influence of many modifying causes; and it is not always easy, even with the utmost care, to avoid all sources of error.

But the opponents of the doctrine assume the less decisive experiments to be the correct ones, and explain the production of the oscillatory motion by the operation of extraneous influences, by the elasticity or tonic contraction of the vessels, by the movements of the animal, or other disturbing causes. When these experiments have been repeated with the variation of removing the heart instead of tying the vessels, the continuance of the capillary circulation has been attributed to the escape of the blood from the divided vessels, and their consequent contraction. Under these circumstances, we repeated the experiments. The following are the notes made at the time:

The medulla oblongata of a frog having been divided, the chest was laid open, and the heart and great vessels were exposed. The web was then placed under the field of the microscope, and the circulation observed. It was apparently in all respects natural. The whole of the heart, with a portion of the vessels, was then cut out with scissors by a friend, while we watched the circulation. Its rapidity was immediately and strikingly diminished, but the blood continued flowing on in the same direction for some few seconds, then the current in the smaller arteries oscillated, moving slowly backwards and forwards, its motion in the veins being very slow, but uniformly progressive. The capillaries seemed gradually to empty themselves into the veins.

Another frog was prepared in the same manner, and a ligature was passed beneath the aorta; while the circulation, which was natural, was observed, the ligature was tightened. The circulation almost immediately became much slower, but continued for more than a minute to move progressively onward with considerable speed, appearing to be in all respects natural, but slow. In between two and three minutes it gradually stopped.

Frogs are readily placed under the influence of chloroform, by causing

* A Dissertation on the Motion of the Blood, &c., by Albert Haller, 1757, translated.

† Deux Mémoires sur le Mouvement du Sang, &c., 1756, expt. 195.

them to breathe its vapour, and they become completely passive and motionless.

Having placed a frog thoroughly under the influence of chloroform, and prepared it, the circulation, which was natural but accelerated, we observed in the web. The blood was unusually florid. The whole of the heart, with the base of the great vessels, was then removed. The rapidity of the circulation was immediately and strikingly diminished; and in a second or two the blood began to oscillate in the arteries, but it continued at the same time to flow progressively, although very slowly, onward in the veins, the capillaries gradually emptying themselves into these vessels. When all motion had ceased, the capillaries contained scarcely any blood, the arteries none, but the veins were much engorged.

A frog was prepared in an exactly similar manner to the last, but the ventricle was removed below the origin of the aorta, so as to leave its valves uninjured. The circulation almost immediately became slower, but continued to flow onward with considerable speed. It was in all respects natural, and no trace of any oscillatory movement was visible. It was quite evident that the rapidity of the circulation was greater in the veins than in the arteries. The capillaries gradually emptied themselves into the veins. Indeed, after the circulation had quite ceased in the arteries (which happened in about two minutes), there was still some movement onward in the veins. During the whole time the frog was perfectly passive and motionless, and the movements of respiration had ceased.

These experiments were frequently and carefully repeated, with a generally uniform result; the chief variation being, that when the whole of the heart, with the origin of the vessels, was removed, the current in the arteries in one or two instances became *immediately* oscillatory, occasionally flowing backwards with some rapidity. But the current in the veins was always progressively, although slowly, onward. These experiments were witnessed on various occasions by several friends, who confirmed their accuracy.

The various results arrived at by different observers may, perhaps, be explained by a consideration of the following circumstances:

It has been remarked that great care is required in the performance of these experiments, in order to guard against the influence of all disturbing causes. Some contradictions may be explained by imperfections in the method of conducting them. Thus Dr. Black states—"The vessels of the web were observed in full circulation immediately before the animal was fastened for the experiment," &c.* Any one accustomed to prepare the frog's web for the microscope must be aware how easily the circulation is interrupted or disturbed.

It is to be observed that there is a striking difference in the effect upon the capillary circulation between excision of the heart, &c., and ligation of the aorta. In our first experiments, we were somewhat puzzled by obtaining two very different results from excision of the heart: sometimes the effect was similar to that produced by ligation of the vessels: at other times, the motion of the blood in the arteries immediately became oscil-

* A Short Inquiry into the Capillary Circulation of the Blood, &c., by J. Black, M.D., 1825, p. 60.

latory. This apparent contradiction we found to depend upon the place where the section was made. Whenever the division was made *below* the valves of the aorta, so as to leave them uninjured, the direction of the circulation was unaltered; but when, on the contrary, so much of the heart was removed as to include the aortic valves, the oscillations before described were immediately observed, for the blood then flowed freely backward through the open orifice. Many of the contradictory statements may be thus reconciled.

With regard to the oscillations which have been observed when a ligature has been employed, we would remark, that great care is required in exposing the heart, and more especially in passing a ligature round the aorta. It is no very easy operation to pass a ligature around the commencement of this vessel without injuring the auricles of the heart. Their delicate walls are very easily torn, and this accident may readily elude observation. No blood should escape if the operation be successfully performed; any hæmorrhage will indicate almost certainly that this accident has happened. Under these circumstances, so much blood is lost as to interfere more or less with the result of the experiment. The natural condition of the circulation is destroyed. The heart ceases to act with due effect upon the blood, for the ventricle does not receive its proper supply.

When these sources of error were guarded against, the results of the experiments appear to be tolerably uniform, whether the ventricle of the heart only be removed below the valves of the aorta, or whether the aorta itself be tied, so that regurgitation of the blood be prevented.

Although it is admitted that the mere fact of something like a circulation still continuing in the smaller vessels after the removal of the heart's influence, cannot be advanced as an unobjectionable argument in support of the doctrine—as it is impossible to say how much is due to the contraction or elasticity of the arteries—yet these experiments, when carefully examined, seem to demonstrate in a striking manner a power supplemental to the heart in moving the blood; and the seat of that power is no less evidently shown by the fact that, when the blood is oscillating in the arteries, its motion is regularly progressive in the veins; or when moving directly onward in the arteries, its rapidity may be observed for a time to be greater in the veins, although the latter vessels are the larger—so contrary to the rule under the influence of the heart's action. The empty state of the capillaries after all movement has ceased, when the heart has been removed or the aorta tied, contrasts strikingly with their engorged condition under circumstances which will be hereafter mentioned. These facts are far more important in connexion with the question than the mere length of time during which the movement of the blood continues after the removal of the heart's influence. It is obvious that the circulation in the capillaries must depend on their supply, and this, from the nature of the experiments, cannot last long.

All extraneous sources of error were guarded against with all possible care. A frog fully under the influence of chloroform is completely passive in all respects. Even the movements of respiration had ceased in most of the experiments.*

* The existence of those pulsating sacs situated upon the primary divisions of the aorta of the frog and toad, described by Dr. M. Hall (op. cit. p. 82), has not been overlooked; but it is obvious that they cannot influence the results of the experiments.

Similar observations have been repeated by Dr. Wilson Philip and others upon the mesentery of warm-blooded animals, but the severe shock occasioned by the operation on them interferes greatly with the result; while the exposure and disturbance of the mesentery, which must inevitably occur, greatly invalidates the results obtained.

"I have long ceased," Dr. M. Hall says, "to place the slightest reliance upon the circulation, as seen in the mesentery, in physiological experiments of any delicacy. The circulation of the web, on the other hand, is unequivocal under judicious management, the arrangement of the toes and of the membrane remaining accurately the same."

The following experiment was devised by Dr. G. Calvert Holland, as "much less exceptionable in its character than any with which we are acquainted, demonstrating the power of the capillaries to carry on the circulation:"

"A placenta was procured, twenty minutes after separation from the uterus, and placed, with the exception of the cord, in a bladder, which was immersed in water at the temperature of 100° Fahr. The free extremity of the cord at the same moment was elevated to an angle of 30°, resting on the edge of a glass, and at the distance of a foot from the placenta. At the commencement of the experiment, no blood escaped from the vein, but in two minutes from the immersion it began to flow, and continued for twenty minutes, and at this time it was found that the glass had received above one ounce."*

Relying, therefore, on the evidence afforded by the experiments which have been related, we cannot subscribe to the following statement:

"Whenever the action of the heart ceases or is impeded, the whole circulation ceases; and that, when an obstruction prevents the action of the heart from reaching the blood in any of the bloodvessels, the flow of blood ceases almost instantaneously in all the branches proceeding from the obstructed vessel."†

3. "After most kinds of natural death, the arterial system is found, subsequently to the lapse of a few hours, almost or completely emptied of blood." Indeed, the contrast between the empty state of the arteries and the engorged condition of the veins after death, is familiar to every one. This state has generally been attributed to the tonic contraction of the arteries which occurs after death.‡ But it will appear, upon consideration, that this explanation will not wholly account for the fact. "The emptying is commonly more complete than could be thus accounted for, . . . since their calibre is not found to have diminished in a proportional degree." Speaking of this assigned cause, Dr. Wilson Philip observes: "This may reduce, but it cannot wholly expel, their contents." This post-mortem contraction of the arterial tubes must be in proportion to the amount of muscular tissue contained in their walls. Now it is well known that, as a general rule, the relative proportion of muscular tissue in the walls of the arteries increases as these vessels diminish in size—the reverse of the arrangement which exists with regard to the elastic tissue; so that the walls of the larger arterial trunks, at a little distance from the

* The Forces by which the Blood is Circulated in the Capillary Vessels, by G. Calvert Holland, M.D.; Edinburgh Medical and Surgical Journal, July, 1842, p. 58.

† Cyclopædia of Anatomy and Physiology: Article, Circulation, by Allen Thompson, M.D., vol. i. p. 655.

‡ See Die Hämodynamik, nach Versuchen, von Dr. A. W. Volkmann, 1850, chap. xii.

heart, are composed almost entirely of elastic, with comparatively but a very small quantity of muscular tissue intermingled; while in the smaller arteries, towards their termination, the muscular tissue is far more abundantly, and the elastic tissue very sparingly, found.* Now, if no other force were in operation upon the blood in the arteries than the muscular contraction of their walls, how are we to explain their "almost or completely emptied" state after death? It is obvious that if it were simply due to this cause, that the blood would be forced backwards into the larger trunks, as well as forwards into the capillaries, by the more complete contraction of the smaller vessels; for no contraction of the larger arterial trunks could, at any time, occur to produce a degree of constriction at all approaching to an obliteration of their cavity. Indeed, the following experiment of Mr. Hunter is an excellent illustration of this point:

"I found in the uterus of a cow, which had been separated from the animal above twenty-four hours, that after it had been injected, and allowed to stand another day, the larger vessels had become much more turgid than when I first injected them, and that the smaller arteries had contracted so as to force the injection back into the larger. This contraction was so obvious, that it could not but be observed at the time, which was forty-eight hours after the separation from the body of the animal."

He continues:

"This shows, too, the muscular power of the smaller arteries to be superior to that of the larger; and that it is probably continued longer after the separation from the body."†

Some very striking instances have been recorded by Dr. Bennett Dowler,‡ who has observed—

"That in the bodies of individuals who have died from yellow fever, the external veins frequently became so distended with blood *within a few minutes* after the cessation of the heart's action, that, when they are opened, the blood flows in a good stream, being sometimes projected to the distance of a foot or more, especially when pressure was applied above the puncture, as in ordinary bloodletting."

Dr. Carpenter, from whom the above is quoted, remarks:

"It is not conceivable that the slowly-acting tonicity of the arteries should have produced such a result as this: which can scarcely, therefore, be attributed to anything else than the continuance of the capillary circulation by forces generated within itself."§

In order to decide this question more completely, the following experiment was performed: Immediately after a woman was delivered, and while the umbilical cord *was still pulsating*, a portion, about six inches in length, was isolated, by means of two ligatures, the one nearest to the placenta being first applied. This portion was, directly afterwards, cut out just within the ligatures, and carefully laid aside. Some blood escaped at the instant of division. The tied extremity of the remaining portion of the cord attached to the placenta was then cut off, and some blood

* See Hunter. Works.

† Hunter's Works, edited by J. F. Palmer, vol. iii. p. 158.

‡ Researches, Critical and Experimental, on the Capillary Circulation, Jan. 1849.

§ Principles of Human Physiology, p. 496.

escaped. In a few minutes the placenta was removed, great care being taken not to handle the portion of the cord attached. This part of the cord was then removed from the placenta, and carefully laid aside. The two portions of the cord were examined twelve hours afterwards. Little or no difference was observed in the amount of blood contained in the veins of the two portions, but the arteries of the portion from the centre contained distinctly much more blood than those vessels of the portion which had remained in connexion with the placenta.

This experiment was repeated many times, with a very uniform result. Mr. Bletchley obtained for us portions of cords treated as above described. Either Mr. Holden or Mr. Coote, who were requested to ascertain the relative amount of blood in the vessels of the two portions, or we ourselves, examined these, and always found a marked difference in the quantity of blood contained in the arteries.

The cords were allowed to remain undisturbed for various periods before they were examined—from six hours to fifty or sixty—in order that the arteries might have ample time for their contraction; Hunter stating, as the result of his experiments—"That the vessels of the cord have the power of contraction above two days after separation from the body."

4. In a careful microscopic examination of the circulation, in its healthy and natural condition, many irregularities may be observed in the speed and direction of the capillary currents, which it seems impossible to ascribe to any influence derived from a "*vis à tergo*." Such variations are often distinctly seen in vessels which are supplied by the same trunk. A striking contrast is often presented by the velocity of the blood in two neighbouring channels. "Sometimes the red particles flow rapidly from one current into a second, as if by attraction." Sometimes the direction of the current is completely reversed, which change is often preceded by a temporary stagnation. Such changes occur more frequently when the heart's action is enfeebled. It has been, moreover, observed, that the velocity of the blood in the capillaries is often greater than "in the trunk, whence several arise, which could not happen in vessels the joint area of which increases as they subdivide, if the only cause of motion were an impulse *à tergo*."* Attempts have been made to explain these changes and variations in the capillary currents, by asserting—

"That variations of pressure, and position, and motions of the animal, are always the causes of these changes. All these variations in the capillary currents are, then, just as in currents of water or irrigated land, merely the results of mechanical causes."†

These explanations will not be deemed satisfactory by any one who has been in the habit of watching these variations in the capillary currents. They are seen when the animal is perfectly passive, and when no pressure or other mechanical causes can be conceived to exist. Such causes certainly often interfere with and influence the capillary circulation, but it is quite possible, with care, to guard against all such sources of error, and these changes occur independently of any extraneous cause which can be conceived. Moreover, an attentive examination of the circulation will

* Outlines of Physiology and Pathology, By W. P. Alison, M.D., 1836. Supplement, p. 22. Haller, Mémoires, &c., p. 561 and expt. 62, 68, 72, 92, 262, &c.

† Müller's Physiology, by Dr. Baly, vol. i. p. 221.

impress the mind with a conviction that the phenomena observed must depend upon causes operating in the part, and are alike inexplicable either by supposing them to result from the action of the heart or vessels, or from the operation of extraneous causes. The following observation throws much light upon this question. It is especially valuable because made by Mr. Wharton Jones, and recorded without reference to the present inquiry:

“Effect of Section of an Artery of the Web of the Frog on the Flow of Blood in the Part.”—When an artery is cut across, it immediately becomes constricted, even to obliteration of its calibre, upwards in the direction of its trunk, and downwards in the direction of its ultimate ramifications. The flow of blood is thus arrested, and the immediate consequence is, an ex-sanguine state of the heart, to which the ramifications of the artery lead. But this state of matters is not of long duration. In the course of a minute or so, relaxation of the wall of the artery and dilatation take place, both above and below the wound. In the upper part of the artery, the flow of blood is re-established as far down as the first considerable branch proceeding from it above the place of section. By this branch the stream of blood passes off. In general, none of the blood, except a stray corpuscle now and then, enters the artery further, although it has become dilated down to the place of section, where, however, the cut end of the vessel continues closed by constriction. Into the part of the artery below the section, blood, of course, no longer enters directly. It enters, however, in a retrograde direction, and very slowly, by one set of branches, and passes out in a direct course, but still very slowly, by another set of branches. The blood which enters the artery below the section, in a retrograde direction, regurgitates from the capillaries and veins to which the branches lead by which the blood enters; and if the cut artery has a direct anastomosis below the section with another artery, blood also regurgitates by that anastomosis.”*

Now, in this experiment, why is the flow of blood re-established in the upper part of the artery *only* so far down as the first considerable branch proceeding from it above the place of section? Why is it that—“In general, none of the blood, except a stray corpuscle now and then, enters the artery further, although it has become dilated down to the place of section?” Can this be explained by any “*vis à tergo*,” or by any mechanical force? Is it due to any other cause than some power in operation at the capillaries to which the vessel leads, which draws the whole of the blood passing through the vessel in that direction?

5. It is well known that there are certain organs which are destined, under different circumstances, to undergo considerable changes in their development and activity. The best examples are—the uterus after conception, the mammary glands during pregnancy and lactation, and the testicles of various animals at certain periods. The increased development of such organs is attended by an increased determination of blood to their tissues, without any change either in the heart's action or in the general circulation. Other instances, in which the flow of blood to particular parts or organs is augmented, may be found wherever there is any increase in the activity of the process of nutrition, secretion, &c. Such temporary changes are of frequent occurrence; but the most obvious examples to be seen in the change which an organ undergoes, when, from disease, its fellow becomes incapable of duly performing its functions.

* On the State of the Blood and the Bloodvessels in Inflammation, &c., by T. Wharton Jones, F.R.S.: Guy's Hospital Reports, vol. vii. part 1, pp. 22, 24. See also Haller, *Mémoires*, &c., expt. 54; and Experimenta circa statum Sanguinis et Vasorum in Inflammatione, auct. G. Kaltenbrunner, 1820; pp. 4, 5, 20, 21.

This often occurs in the kidney. The structure of one of these organs becomes damaged, and the function correspondingly impaired. Thus additional work is thrown upon the remaining healthy one,—its tissue is considerably increased,—it becomes hypertrophied, and the flow of blood to it is proportionally augmented. Moreover, it is to be observed that under these circumstances not only is the quantity of blood passing to the part or organ augmented, but its circulation is more rapid.* Now, upon what cause does this increase in the quantity of blood, and in the rapidity of its flow, depend? Obviously not upon any change in the force of the heart's action, for this would affect the system generally. Indeed, there is no relation between the activity of the heart and the circulation in certain parts. "The distribution of the blood is not in the least influenced by the heart."† The cause of these local determinations of blood has been sought for in the arteries.‡ It has been supposed by some to depend on an increased action of these vessels. But the only active power possessed by arteries is contraction, due to their muscular tissue. This could only act to constrict their calibre, and so diminish the quantity of blood passing through them. Those obscure notions about a vermicular contraction need no reply. But attempts have been made by high authorities (Magendie, Mayo) to explain these local determinations of blood by a relaxation of the arteries leading to the part, and a consequent diminished pressure on the blood. Such dilatation may possibly accompany such changes, but they cannot be regarded as the cause. If this were so, various irritations which excite local determinations of blood "must act as *sedatives* on the contractile power of the arteries; which is not only the reverse of their action on more strictly irritable textures, but is the reverse of the effect which they have been often observed, in experiments, to produce and keep up, for a considerable time, on individual living arteries, to which they have been applied."|| Moreover, local determinations of blood are often the "consequence of a strictly local cause;"§ and it cannot be conceived how this can act to produce relaxation of an artery. When, for instance, the capillary circulation is examined under the microscope, it is observed that its rapidity is increased by moderate stimuli and retarded by sedatives;¶ and these experiments afford an additional proof that the cause operates at the capillaries, and not elsewhere.

Again, relaxation of an artery could not explain the increase in the rapidity of the flow of blood through the vessel; indeed, this cause alone would tend to retard it. Moreover, if this local determination be kept up for any length of time, as in the uterus or mamma, the arteries leading to such parts undergo themselves an increase of development—they become enlarged, thickened in their walls, and tortuous. The subsidence of this increased activity in a part or organ is attended with a corresponding

* See Wharton Jones, op. cit.

† Volkmann, op. cit., p. 341.

‡ Because variations in the capillary circulation are of course accompanied by corresponding variations in the flow of blood through the arteries supplying them, it has been assumed that the arteries are the "regulators of the capillary circulation." Are not the cause and effect here reversed?—See section vi.

§ Allison, op. cit., supplement, p. 20.

|| Carpenter: Principles of Human Physiology, p. 497.

¶ Wilson Phillip, op. cit., 286—6. Also, an Inquiry into the Nature of Sleep and Death, 1834, p. 72.

diminution in the amount of blood passing to it. It seems equally impossible to explain this by supposing the contraction of its arteries to be the cause. "It is difficult to understand how the heart is not able to maintain this increased flow of blood after the periods of activity in the functions of nutrition and secretion have ceased, if we look for an explanation of this in any property possessed by the coats of the blood-vessels."*

In the consideration of local determinations of blood, mere evanescent changes are generally included—such as the act of blushing, &c.—also the changes which ensue in erectile organs or tissues. Now neither of these states has been brought forward in the previous remarks, because they appear to depend on very different causes, or at all events they are open to certain objections which will not apply to the other cases. For instance, the evanescent phenomena of blushing seem to be immediately dependent on some "nervous influence," which is at present involved in considerable obscurity; while the phenomena of erection appear to be clearly due, in a great measure, to some special and peculiar structure of the parts in which they occur.

If, then, the cause of the changes which have been considered can be found neither in the heart nor arteries, it must be sought for in the part itself. "If more blood is carried to one organ than to another—i.e., more than the normal width, length, form, and arrangement of its vessels justifies, we have to deal with a case in which we must seek for the explanation in new moving forces."† The nature of these forces will be hereafter inquired into, but they must be conceived from the previous considerations, to operate upon the blood while in the capillary vessels, and so to affect it as to increase its local determination and the rapidity of its motion.

6. The proofs upon which this statement rests are to be found in the arguments advanced in the previous section, and also in those contained in sections 4 and 7.

For it is proved that the determination of blood to any organ is the result and not the cause of the local action. This statement is well illustrated by the different changes which occur in the arteries of a limb, a portion of which has been amputated, and where a ligature has been placed on the main trunk for the cure of aneurism.

"When an artery is tied after amputation, the end of the vessel, although exposed to the influence of the heart and arteries, does not become enlarged, nor are the branches given off by it above the ligature dilated. In the dissection of several stumps I have invariably found a considerable extent of the main artery, as well as the branches arising from it, remarkably contracted."‡

Contrast this account with the following:

"The dilatation of the vessels, by which a collateral circulation is carried on, takes place principally in the minute ramifications. The trunks of the branches from which these ramifications originate are very inconsiderably enlarged, even when a collateral circulation has been for a long time established. In several preparations which I have examined at different periods after the artery had been

* *Physiological, Pathological, and Anatomical Researches*, by John Reid, M.D., 1848, p. 47.

† *Volkmann*, *Op cit.*, p. 341.

‡ *On the Diseases of Arteries and Veins*, by Joseph Hodgson, 1815, p. 253—4. See also Wharton Jones' *expt.* before quoted, pp. 32, 33.

tied, the mouths of the branches above the place of obstruction in the main artery did not appear larger than in their natural state, and in a few instances only a slight dilatation was perceptible." (p. 247.)

Whence this difference? Why, when a portion of a limb is removed, are the vessels considerably diminished in size, while when the whole limb remains the collateral branches become enormously enlarged? It is especially interesting to mark the vessels in which this dilatation occurs—to observe that it "takes place principally in the minute ramifications," because it amply refutes all notion that the enlargement of the vessels is the result of mere distension "in consequence of the obstruction," or that it can be due to any *vis à tergo*. The influence of the cause is manifestly *retrograde* along the vessels.*

Again, how can we explain the difference between the vigorous flow of blood in the vessels of a healthy, well-nourished limb, and the languid circulation in one that has been long kept at perfect rest—as in a state of paralysis, or of some chronic disease of a joint—if not by admitting that the quantity of blood passing through the arteries is regulated by causes operating in the part?

When the placenta is detached from the uterus, with the fœtus, at birth, the pulsation of the cord ceases first at the placenta, and afterwards at the umbilicus. After such pulsation has ceased, section of the cord is followed by the escape of comparatively very little blood; in many instances, by none whatever.† Whence this change? Why does the blood thus desert the umbilical cord? The heart and other forces operating from behind are not less active. Indeed, the contrary is the fact. The case is a remarkable one, and seems susceptible of but one explanation.

Admitting, then, these facts, it is clear that the local causes which can regulate the quantity of blood passing through the arteries must act by influencing the circulation in the parts supplied by them. The nature of this influence will be subsequently considered.

7. Perhaps the best illustration of this statement is afforded by the changes which ensue when the process of respiration is impeded or arrested. There are few subjects about which more controversy has arisen, or which have more fully engaged the attention of a greater number of eminent physiologists, than the pathology of asphyxia. Since the time of Haller, doctrine after doctrine has been advanced and refuted. But at length it appears to have been clearly proved, that the first of the morbid changes is an obstruction to the passage of the blood through the capillaries of the lungs, and that this obstruction is due to the cessation of the chemical changes between the air and the blood—to the cessation of the changes which normally occur in the blood during its passage through the capillaries of the lungs. It cannot be necessary now to retrace the various facts and arguments upon which this conclusion is founded. The task has been already repeatedly performed, and various objections which have been occasionally advanced have been satisfactorily answered. As to the relation the morbid changes which follow this

* Alison, op. cit., p. 30. Reid, op. cit., pp. 47, 48.

† On the Circulation of the Blood in Acardiac Fœtuses, &c., by John Houston, M.D.: Dublin Journal, 1843, vol. xxiv. pp. 248—9. Graves's Clinical Medicine, 1843, pp. 482—3.

primary one bear to each other, our knowledge is less clear and certain, but with these we are not, in the present instance, concerned: it is simply with the fact, that the circulation of blood through the capillaries of the lungs is obstructed in proportion as the changes which it naturally undergoes in those organs are prevented.

Some objections which have been advanced against this statement by Mr. Erichsen* require notice. The first of these objections is—

“That provided the heart’s action be maintained, black blood may be made to circulate through a lung (in which the chemical changes have entirely ceased) for a much longer period than in ordinary cases of asphyxia, the same force that keeps up the circulation of red blood in one lung sufficing for that of black blood in the other.” (p. 24.)

And this statement is founded upon two experiments, which are detailed.† In these experiments—

“A tube was adapted to the trachea of a young spaniel. The animal was immediately pithed; artificial respiration was then set up, so as to maintain the heart’s action, and the chest was laid open. A ligature was next passed under the right bronchus, close by the bifurcation of the trachea, and tied tightly, so as to prevent the entrance of any air into the right lung. This was done in about three minutes from the time that the animal was pithed. The inflation, which was only carried on by the left lung, was now continued—the right lung remaining in a mid state between collapse and distension, and quite motionless. The heart was beating tumultuously—about 120 per minute.”

In the first experiment—

“9th minute after ligature of bronchus: heart beating forcibly, but rather irregularly, from 60 to 70. A ligature was now passed under one of the pulmonary veins of the right lung (the obstructed one), and tied as near as possible to its entrance into the auricle. The vessel was then punctured on the distal side of the ligature, and a jet of semi-venous blood escaped. The blood was not quite black, owing to regurgitation having taken place from the left auricle, as the pulmonary vein was not ligatured until after inflation had been established for several minutes. . . . 11th minute: a quantity of venous blood trickles out of the puncture in the pulmonary vein.”

Up to the 17th minute, black blood continued to ooze from the puncture, slowly, but very distinctly. In the second experiment—

“At the 10th minute after the bronchus had been tied, the heart was beating 60 to 64, strongly and regularly; artificial respiration was kept up with the left lung alone, the right one being quite collapsed. . . . 14th minute: A ligature having been passed under a pulmonary vein of each lung, these vessels were punctured on their distal side. A quantity of black blood flowed from the pulmonary vein of the right lung, whilst from the corresponding vessel of the left lung florid arterial blood escaped in a small jet. . . . 17th minute: From the pulmonary vein of the right side dark blood is still flowing, rather slowly, but very distinctly; whilst from that of the left lung a larger quantity of arterial blood is evidently escaping. . . . 19th minute: Flow of blood still continues, but very slowly. . . . The blood that had accumulated in either side of the chest was carefully collected and measured, when it was found that that which had flowed from the right lung amounted to $2\frac{1}{2}$ drachms, while that from the left amounted to $3\frac{1}{2}$ drachms; and in another experiment of the same kind the quantities that escaped were respectively $2\frac{1}{2}$ and 4 drachms.”

* *An Experimental Inquiry into the Pathology and Treatment of Asphyxia*, by John E. Erichsen: Edinburgh Medical and Surgical Journal, Jan. 1845.

† Experiments 10 and 11.

What evidence do these experiments furnish that "the circulation of black blood may continue through the lung for a considerable time beyond the period at which the circulation ordinarily ceases in asphyxia?" In twenty-four minutes the quantity "which had flowed" amounted to $2\frac{1}{4}$ drachms. That so small a quantity as $3\frac{1}{2}$ drachms only flowed from the left lung may, we think, be easily accounted for by the nature of the experiment. Artificial respiration was set up, and *the chest was laid open*. We can scarcely imagine the functions of either lung to be continued, to any great extent, under these circumstances. But it must also be remembered—and this fact is generally overlooked in all such experiments—that "when the access of air to the lungs is suddenly and completely checked, the circulation through them continues for some little time," because

"A considerable quantity of air is contained in the air cells of the lungs; and that it is not until this has been so far deprived of its oxygen, and loaded with carbonic acid, as to be unfit to effect any change in the blood, that we should expect the movement to be entirely checked. Moreover, the alteration in the character of the whole mass of the circulating fluid is effected gradually, as might be inferred from the small proportions transmitted by the heart at each contraction; so that, if a small stream be drawn from the carotid artery of an animal undergoing asphyxia, it will be seen to become progressively darker from the commencement of the suspension of the respiratory movements to the cessation of the heart's action."*

This important fact did not escape the attention of Bichat, who proves it thus:—If the access of air to the lungs be suddenly checked immediately after an inspiration, the change of colour in the blood occurs more slowly than if the supply of air be arrested immediately after an expiration, and the change is especially rapid after a forced expiration; or if, by means of a syringe adapted to the tube in the trachea, the air be drawn out of the lungs, the colour of the blood passes from red to black very suddenly—twenty or thirty seconds suffice for the change. Whereas, if air be forced into the lungs, beyond what is taken in by the deepest inspiration, and retained there, the blood remains of its natural colour for a much longer time—it does not become darkened for more than a minute—it does not flow out completely black till the end of three; and this varies according to the state and the quantity of air that is thrown in.†

Again, it must be very difficult to tell when the blood has become "perfectly venous," and has passed through the capillaries of the lungs independently of any change. Certainly, mere inspection is very unsatisfactory.‡ That it continues to circulate so long as any change occurs, but only in a degree proportionate to the extent of the change, is a very conclusive proof of the truth of the doctrine.

Mr. Erichsen appeals, also, to "the fact of the circulation continuing actively in lungs that are compressed by effusion into the pleural sac," as evidence "that the heart's action is of itself sufficient, when vigorous, to keep the circulation through a lung in which the chemical changes have

* Library of Medicine, vol. iii.: Article, Asphyxia, by Dr. Carpenter, p. 225.

† Recherches Physiologiques sur la Vie et la Mort, par Xavier Bichat, 1805, pp. 241—2.

‡ This is strikingly illustrated by the contradictory statements which have been advanced as the result of actual observation.—See Bichat, loc. cit.; Kay, op. cit., p. 188, &c. &c.

ceased." (p. 24.) Conclusions drawn from such data must be acknowledged to be, in the highest degree, uncertain. How can we estimate, in any way, the amount of blood passing through the compressed lung, and when can we say that the chemical changes have entirely ceased?

The other objection is the theory,

"That the obstruction which has been proved to take place in the pulmonary and systemic circulation, is due to the venous blood exciting the contractility of the minute divisions of the arteries and pulmonary veins, by acting upon their special sensibility." (p. 31.)

The arguments which Dr. Reid has opposed to this theory appear to be conclusive; for he shows that the obstruction cannot be caused by the venous blood acting as an excitant upon the contractility of the ultimate ramifications of the pulmonary veins, because incompatible with some well-established facts. The contractility possessed by the vessels manifests itself by slow contraction, followed by equally slow relaxation.

"When contraction has been induced in the coats of an artery by an excitant, it is well known that relaxation does *not follow suddenly*, even on the withdrawal of an excitant, but it occurs slowly and gradually. If, then, the ultimate ramifications of the pulmonary veins are stimulated to contraction by the venous blood, relaxation ought *not to follow instantly* upon the withdrawal of this excitant, and the entrance of pure atmospheric air into the lungs ought not to be instantly succeeded by the free passage of blood from the right to the left side of the heart. The following experiment, mentioned by Bichat, and which I have frequently repeated, appears to me to be an *Experimentum crucis* upon the point under discussion. If a tube with a stop-cock upon it be tied into the trachea of an animal, and the stop-cock turned to exclude fresh air from the lungs until the circulation of dark blood along the arteries has become much enfeebled, as ascertained by exposing a large artery and making an opening into it, *instantly* on the admission of fresh air into the lungs the blood springs from the cut artery of a bright red colour, and with greatly increased force. I have observed the same thing repeatedly when the hemadynamometer was fixed in the femoral artery; no sooner was the stop-cock opened, and fresh air permitted to enter the lungs, than the mercury suddenly sprung up several inches in the ascending portion of the tube of the instrument."*

Thus, then, the passage of the blood through the pulmonary artery is impeded by any cause which interferes with the normal changes which the blood undergoes in the capillaries of the lungs.

But the above is not the only illustration which can be drawn from the morbid changes which occur in asphyxia. Dr. Reid has shown most conclusively, that if the blood becomes venous in the arteries, it will not circulate freely through the systemic capillaries; that it is obstructed in its passage through these vessels, causing increased pressure on the walls of the arteries. These experiments appear unexceptionable. Again, Dr. Kay relates an experiment which shows that it is very much more difficult to force venous than arterial blood through the capillaries of the lung. The same fact is proved by the experiments of Mr. Wharton Jones, who caused the blood to stagnate in the capillaries of a frog, by directing upon them a stream of carbonic acid gas.† Also the fact before

* Dr. John Reid, *op cit.*, pp. 45—6.

† British and Foreign Medical Review, vol. xiv p 600.

mentioned, that the circulation in the placenta, and therefore the flow of blood to it, ceases when it is detached from the uterus, may be here again referred to. •

Dr. Allen Thompson has said,

"We are not acquainted with any facts or experiments which show that the systemic capillary circulation is immediately dependent upon the change of the arterial into venous blood. On the contrary, such an opinion is, opposed by the facts—that a free circulation of imperfectly arterialized blood takes place in the fœtus before birth, as well as in many children after birth, affected with malformation of the heart or greater vessels; and that a completely venous blood circulates through the system in hibernating animals, when in the state of deepest torpidity."*

Now the facts here referred to are very important, because they are very closely connected with the question. If it can be proved that the blood continues to circulate through the capillaries independently of *any* change, then assuredly an incontrovertible argument is advanced against the doctrine under consideration. But, on the other hand, if it can be shown that a change, however slight, does occur, and that the activity of the circulation is in direct proportion to the extent of this change, then, on the contrary, we have an irresistible argument in support of the doctrine.

To say "that a free circulation of imperfectly arterialized blood takes place in the fœtus," is hardly stating the whole of the case. We know that in the fœtus different parts receive blood of a different character, and we have also good evidence for believing that the activity of the circulation varies accordingly; and moreover, that in the fœtus the blood, as a whole, undergoes a most decided change; and that upon this change the life of the fœtus directly depends, there can be no doubt. "In many children after birth, affected with malformation of the heart or greater vessels," we certainly witness the circulation of imperfectly arterialized blood, as in cases of cyanosis. But is it "free"? On the contrary, a languid circulation is most characteristic of the disease, and its torpidity strikingly corresponds with the deficiency in the change of the blood. Lastly, what is the condition of the respiration and circulation in hibernating animals? Dr. Marshall Hall, who has most closely investigated this subject,† in his account of the extent to which the respiration is suspended, says, in different places:—"The respiration is very nearly suspended in hibernation." (p. 769.) "I think it right to remark, that after the apparent total cessation of respiration . . . there is probably still a slight diaphragmatic breathing." (p. 769.) Then again he says, "although the respiration be suspended," &c. (p. 772.) "The respiration is nearly, if not totally, suspended." (p. 772.) "In the midst of a suspended respiration," &c. (p. 772.) That the changes which the blood naturally undergoes in respiration are, in animals in a state of hibernation, nearly suspended, is proved not only by the analysis of a confined portion of air in which they have kept for a considerable time, but also by the fact that they can bear the total deprivation of oxygen, as by immersion in water, for a considerable period. But that some change is effected in the blood seems equally proved by the fact, that they cannot wholly

* Op. cit. p. 677.

† *Cyclopædia of Anatomy and Physiology*, vol. ii.: Article, Hibernation.

dispense with the presence of oxygen. If no change occurred in the blood while passing through the capillaries of the lungs, animals, while hibernating, ought to be entirely independent of the presence of atmospheric air, for any length of time, while hibernation lasts; but this, we know, is not the case. Indeed, it cannot be doubted, however deficient the respiratory movements may be, that the blood undergoes some change in its passage through the capillaries of the lungs.

Now, what is the condition of the circulation in an animal in a state of hibernation? The same authority says, "The circulation is reduced to an extreme degree of slowness, according to a law well known, but hitherto, we believe, unexplained: according to which the respiration and the circulation are always proportionate to each other." (p. 771.) What clearer evidence could be furnished in support of any doctrine than this—that the activity of the circulation varies with the extent of change which the blood undergoes in the capillaries? And does not this doctrine furnish a satisfactory explanation of the law "hitherto unexplained?" A law generally observed under the various conditions of health, and often still more strikingly illustrated in disease. Indeed, if this doctrine be true, it is apparent that, within certain limits, the respiration and the circulation would be proportionate to each other.

During hibernation the condition of the animal is reduced below that of the reptile; but the analogy is obvious. In reptiles we are more familiar with the fact, that a darker blood circulates more languidly throughout the system. They may with truth be said to live more slowly; yet we find in them, also, the same laws in force, modified, but not annulled. Neither here, nor elsewhere, does the blood flow through the capillaries independently of change.

We conclude here our discussion of the several arguments in favour of a special local force in the capillary circulation; and in the next number we shall consider the opposite arguments, and complete our review of the subject.

(To be continued.)

William S. Savory.

REVIEW IX.

On the Formation and Extension of Cancer-Cells in the Neighbourhood of Cancer, and their Importance in the Performance of an Operation.

By J. L. C. SCHROEDER VAN DER KOLK. ('Nederlandsch Lancet,' Sept. 1853.)

THE paper by the eminent Professor of Utrecht, which we have quoted above, appears to us to be of such interest, that we have thought it advisable to give a very full abstract of it, and as often as possible to use the author's own words.

"Among the various diseases to which the human frame is liable," says the author, "none have ever been looked on as more formidable than the so-called malignant tumours, which exhibit themselves in their several stages as scirrhus, carcinoma, and medullary fungus; not only on account of the insufficiency of art to contend successfully against their development and extension, but especially by reason of the great uncertainty which exists, that the removal of these diseases by

operation shall be permanently successful, since in most instances they break out again, after a shorter or longer interval, with renewed violence, and usually bring the unhappy sufferer to a painful and torturing end.

"Although these diseases have of late years been much and amply treated of by various writers, by whom their structure and development have been more accurately investigated under the microscope, I think every contribution, however trifling, to a more accurate knowledge of the origin and dissemination of maladies which so obstinately resist all remedies, and defy the efforts of the healing arts, sufficiently important to excite general attention and closer investigation."

• Having made these prefatory remarks, the author alludes to some observations on the origin, development, and dissemination of these diseases, communicated by him in the year 1847 to the sectional meetings of the Provincial Society of Utrecht,* where he showed, that around a carcinomatous tumour, in tissues which, to the naked eye, appear to be perfectly sound, the disease becomes developed under the form of little cells, gradually increasing in size, and collecting in larger and more numerous groups in proportion as we approach the swelling; and, in some cases, in advanced stages of the affection, extending tolerably far into the surrounding parts.

"Without going," continues the author, "into any detailed considerations, I shall briefly communicate my observations on the subject, in the first place, in reference to the spread of epithelial, and subsequently to that of ordinary, cancer.

• "If we divide an extirpated underlip, more or less affected with epithelial cancer, in such a manner as to obtain an anterior and posterior portion, we shall be able, with tolerable accuracy, to observe the extension of the disease, so far as we can follow it with the naked eye. Generally speaking, the disease has a greater tendency to spread along the edge of the lip than downwards in the direction of the chin. If we now, with a sharp knife (I generally use a razor for the purpose), take a very thin layer from the morbid portion of the section, we shall see that the disease is formed by an assemblage of epithelial cells. These become smaller as we approach the boundaries of the affected part. If we now accurately examine the neighbouring parts, which still appear sound, under the microscope, we shall find therein (in quantity varying in proportion as we remove further from the affected parts, and go deeper into the apparently sound tissue) little cells, nuclei, and, finally, granular matter and fat-corpuscles, scattered among the healthy tissue, so that we can never observe the progress and development of these cells more accurately than in these very tumours; in fact, we can often most clearly see, in the same microscopic field, the entire progress of the transition of nuclei, around which a cell has as yet scarcely formed, to the fully-developed epithelial-cell; and with a strong power, we may observe a soft more or less granular matter being deposited around the nuclei, and cells forming precisely in the manner represented by Schwann."

Investigating more closely their diffusion in the neighbouring parts and muscular fibres, the author found the nuclei and cells to extend principally along the areolar tissue between the fibres, so that in some parts of this tissue, little groups of cells had collected, while on the fibres themselves, several nuclei and little cells lay scattered. In other places, granular matter and nuclei, in the stage of first formation, alone were to be seen in the tissue, while scattered among them were fat-corpuscles, easily recognisable by their darker contours.

* Aanteekeningen in de Sectie-vergaderingen van het Prov. Utr. Genootschap, 1847, pp. 27, et seq.; also translated into Swedish, by Dr. Wahlgren.

"In the muscular fibres themselves, it was difficult to trace any alteration; even when they were tolerably abundantly covered with nuclei and little cells, the transverse striæ could still, if sufficient light was employed, be perceived. In some places these, however, disappeared, and seemed to be replaced by longitudinal striæ, presenting a more fibrous aspect; in individual cases, I even succeeded in recognising a dissolution and degeneration of the muscular fibre itself, the latter appearing to be resolved into transverse divisions or cells, intermingled with numerous fat-corpuscles; in fact, the fibre seemed to have undergone a fatty metamorphosis."

The author could not, with any certainty, perceive degeneration in the nerves in these cases of cancer of the lip: these organs appeared long to resist the disease. In several places he even found bundles of nerves entirely surrounded with epithelial cells, while within the neurilemma no trace of any degeneration was visible; it was only in isolated spots that he now and then observed nuclei and little cells between the nervous fibrillæ, but in much smaller number than between the muscular fibres; in a few instances he saw solitary fibrillæ, covered with epithelial cells, passing out through the connecting tissue. In other cases of carcinoma, however, he frequently met degeneration of the nerves, as shall be mentioned hereafter.

An unusual number of epithelial cells and nuclei was also present in the follicles and crypts of the beard. Omitting, however, a more detailed description of epithelial cancer and the changes it gives rise to, the author passes to the consideration of the more immediate subject of his paper—namely, the extension of the degeneration when the disease has been once established.

"It is quite unknown," he observes, "what may be the cause of the extensive cell-formation by which new cells appear constantly to form in the parenchymatous fluid pervading the parts which are still sound, from little nuclei and fat corpuscles, gradually assuming the form of epithelial cells. It may, however, be looked upon as certain that, between these cells and the interstitial fluid, a considerable interchange of material takes place, so that the fluid present in the tumour between the cells acquires other elements than exist in the parenchymatous fluid of the neighbouring sound parts. Both fluids, however, meet at the boundaries of the tumour, and must intermix; and thus, in my opinion, can we best explain how nuclei and cells form in the surrounding tissue as well as in the tumour itself, where the number of these cells increases so much."

"As, however, the parenchymatous fluid penetrates all parts, principally in the course of the areolar tissue, it is evident why the new formation of nuclei and cells takes place between the muscular fibres and other structures chiefly along this membrane, and thence extends into the adjacent parts. These parts, however, for a tolerably long time, offer resistance to the injurious influence and pressure of the cells constantly accumulating in the interstices, so that we cannot discover the least change of tissue with the naked eye, although the microscope shows us that a greater or less formation of cells has already penetrated between the healthy structures."

The author first discovered the importance of accurately tracing this cell-formation in an extirpated lip sent to him for microscopic examination. In this case, although the tumour, which consisted entirely of epithelial cells, appeared to the naked eye to be surrounded to the extent of one and a half or two lines with healthy structures, he observed, on microscopic examination of a very thin layer of the edge of the removed portion, a number of granules, nuclei, and already-developed epithelial

cells, from which he inferred that the edges of the wound of the portion of the lip left behind should contain the seeds of the disease, that the latter had been incompletely removed, and that a relapse was inevitable; an opinion which was verified within three months—notwithstanding that the wound had healed rapidly and favourably—by the return of the disease, which, after causing fearful destruction, terminated in the death of the patient.

When we examine the nature of these cells, we shall see that it is not likely that, when once formed, they will be removed by the inflammation consequent on the operation, especially as the wound made in extirpating cancer of the lips generally heals by the first intention. The latter fact almost excludes the possibility of the destruction by suppuration of the cells and nuclei which have been left behind—

“And it will, therefore, depend on the degree of development of these cells, and on the greater or less amount of subsequent irritation and access of fluids, and on the interchange of material thus brought about, whether, after the healing of the wound, these cells shall again begin to be developed; forming, on a greater scale and with renewed violence, a carcinomatous ulcer, which, generally speaking, is no longer amenable to treatment. It is, however, not less important to note that the parenchymatous fluid, which, in the cancer of the lip and the parts adjacent, is already full of cells and nuclei, and is tainted through the interchange of material, not only gradually penetrates and extends still further into the surrounding parts, and thus gives rise to a constantly spreading and serious ulceration, but is also taken up by the lymphatics, whence arise similar lesions in other and sometimes distant situations. Although this observation applies much more strongly to true cancer, we are not without examples of it in the epithelial disease. Thus, my friend Professor Donders mentioned to me that, in a case of cancer of the lip, he had found the glands in the neck, next the larynx, swollen, and quite filled with epithelial cells. Lebert, Bennett, and Hannover have remarked a similar extension of epithelial cells to neighbouring glands. In epithelial cancer of the bladder, Schrant found cavities in the humerus and vertebra filled with epithelial cells; a cavity also occurred in the os frontis. Virchow describes similar cases. These observations are, indeed, very important, as demonstrating the absorption of the parenchymatous fluid, and the tendency to the formation of epithelial cells, thence derived. Whether the latter might be explained solely from the chemically-altered constituents of the infected and absorbed parenchymatous fluid, or whether we must assume that some of these minute nuclei or little cells have forced their way into the lymphatics, may appear doubtful, as even blood-corpuscles, which are larger, occasionally occur in these vessels, having entered in another manner. It, however, seems to me probable that we must refer this cell-formation in the lymphatics to an alteration in the parenchymatous fluid, since, as we shall hereafter see, the cancer-cells may even form within a structureless membrane (sarcolemma) surrounding the muscular fibres, and through which no nuclei can penetrate. In a case of epithelial cancer of the tongue, I found not only that the epithelial cells had penetrated deeply between the muscular fibres, to the root of that organ, but that a carcinomatous tumour had at the same time formed in the neck, passing into open cancer, under which the patient sank, and in which I found ordinary cancer-cells, but no epithelial cells; whence it would appear that in epithelial cancer, after the absorption of the parenchymatous fluid, a general infection and secondary cancer-cells of a different kind may arise, which sufficiently demonstrates the analogy of the two affections.* The diffusion of cells which occurs in epi-

* Some writers, as Lebert (*Physiol. Pathol.*, § 2, p. 615) and Hannover, deny that epithelial cancer can degenerate into true cancer, or that the one disease may give rise to the other. Hannover considers that even where true cancer supervenes in epithelioma, only a combination of the two diseases had existed, as epithelial-cells cannot pass into cancer-cells. (*Das Epithe-*

thelioma, in the parts adjacent to the swelling, takes place in no less a degree in ordinary cancer and medullary fungus, which is only the acute form of the same disease.

"In ordinary cancer, as well as in epithelioma, the cell-formation around the tumour follows the direction of the parenchymatous fluid which is contained in the areolar tissue, and in this way it penetrates the textures of the surrounding organs. I perceived this very plainly, among other instances, in the case of a woman nearly eighty years of age, who had a medullary fungus in the calf of the leg, by which the gastrocnemius was in great part destroyed. In many places the muscular fibres could no longer be recognised, in others the texture was still plainly visible. On microscopic investigation, it appeared that in some of the parts most affected the primitive muscular bundles had entirely lost their structure; a fibrous form was indeed still to be perceived, but it exhibited itself only as a mass of cells and nuclei, more or less linked together longitudinally, and in many situations enclosed in the unaltered sarcolemma; in other parts the primitive bundles were still entire, exhibiting even their transverse striæ; but they were very transparent, charged, to a greater or less extent, with fat corpuscles, and covered with some cells under the sarcolemma.

"It shortly attracted my attention, what a very important alteration the primitive bundles had in many places undergone,—the fibres being changed into fibre-cells of various lengths, which, now lying closely packed on one another, and again in diminished number, were enclosed in the loose-enveloping membrane, the sarcolemma."

The author, in an admirable figure, copied from a drawing of his own, gives a representation of the successive stages of the destruction of the muscular fibres contained in their loose sarcolemma; in a note he suggests that

"The long membranous filaments which Schrant found in a medullary tumour under the glutæus muscle, and which were more numerous in the vicinity of the muscle, and even exhibited transverse striæ, from which the tube acquired a surprising resemblance to sarcolemma,* were not new formations, but similarly degenerated muscular fibres, where, after the disappearance of the fibres, the sarcolemma alone remained as a thin membrane. Perhaps also some of the tubular fibres and pipe-like structures, which Rokitansky describes as new forms, are of a like nature."

In the text he remarks, that from the description he has given in explanation of the plate,

"It appears that, by transudation, or imbibition, the morbid parenchymatous fluid is taken up by the sarcolemma, and now the same cell-formation sets in within the sheath as had previously taken place in the surrounding areolar tissue. The parenchymatous fluid must thus, by interchange with the contents of the cancer-cells, undergo such a change, that it everywhere acquires the tendency to produce similar nuclei and cells. When these cells are once formed within this thin sheath, and are in contact with the primitive fibre, normal nutrition can no longer proceed; the muscular fibre becomes altered, and divides, under the form of longitudinally extended cells, into several parts, not unlike the fibres of invo-

lioma, Leipzig, 1852, p. 21.) Bennett, on the contrary, says that epithelial-cells, especially new ones, may exhibit all the characteristics of true cells. (On Cancerous and Canceroid Growths, p. 149.) It is, however, another question whether this parenchymatous fluid of epithelioma, when taken up by the lymphatics, and perhaps by bloodvessels, may not be so much altered as to give rise in other situations—for example, in glands—to the development of true cancer-cells, although, in other cases, epithelial-cells form in the glands. It seems to me that the occurrence of true cancer after epithelioma of the lips can scarcely be otherwise explained; Schrant, too, thinks that the epithelial form may produce true cancer. (See Schrant, *Prysverhandeling over de goed- en kwaadaardige gezwellen*, blz. 396, 349, et seq.)

* Op. cit., p. 352, et seq.

luntary muscles, and finally becomes entirely broken up and replaced by a constantly increasing quantity of cells. The sarcolemma appears for a very long time to resist destruction. In some places I could still plainly observe it where the contents already appeared to consist solely of cells, and no longer exhibited a fibrous structure.

"Not less was the effect of these cancer-cells upon the nerves. This I particularly had an opportunity of observing in the carcinomatous tumour of the leg of the old woman, the state of whose muscular fibres I have already described, where the fibular nerve was greatly affected, and surrounded with medullary fungus. The microscopic condition of the nervous bundles and fibres was very variable. In the parts most affected no primitive fibrillæ could be perceived, and the nerve appeared, from atrophy of the tube, to consist merely of connecting tissue; in other places the change could easily be observed."

Some fibrillæ were very broad, especially in particular parts, and were almost entirely filled with cells and fat globules; others were much atrophied, and in some portions devoid of cells; while others, filled and distended at one part with cells, suddenly collapsed at another. In the connecting tissue situated between the fibrillæ could be seen portions of atrophied nervous fibrillæ, the remainder of which appeared to have been changed into fibres or connecting tissue. The fibrous tissue between the primitive fibrillæ itself contained few or no cells, and probably consisted for the most part of atrophied nervous fibrillæ; the connecting tissue, however, which surrounded the nervous bundle externally, was completely studded with cancer-cells similar to those which occupied the fibrillæ themselves. From these circumstances, the author argues, that the tissue situated between the fibrillæ consisted rather of atrophied fibrillæ than of original areolar tissue, an hypothesis which would also explain the small number of fibrillæ found in a nervous bundle of such thickness. Several bundles moreover appeared to consist of thin filaments, as connecting tissue, without containing a trace of nervous fibre. Bennett, too, has noticed this disappearance of the nervous fibrillæ, and has represented them as being changed into fibres, with numerous fat globules and granular matter.

"It cannot, however," observes the author, in a note, "be ascertained with certainty whether the cells found in the nervous tube are true cancer-cells, since these tumefied nerves entirely agree with the change we meet with in a divided nerve. Since, however, the cancer, and not any mechanical pressure on the nerve, which did not exist, was, in this instance, incontestably the cause of the nervous degeneration, and at the same time similar cells had formed, as we have seen, within the sarcolemma of the muscular fibres, it appears to me extremely probable that true cancer-cells had been developed in the nervous tube, which had caused the atrophy, which latter can scarcely be explained in any other way."

The author has observed a similar extension of cancer-cells in the internal parts of the body. Thus, among other instances, he was able, in a case of cancer of the pyloric extremity of the stomach, plainly to follow the progress and diffusion of the cancer-cells: in the apparently still healthy portions of the muscular fibres of the stomach he found several small cells and nuclei interspersed. He also witnessed a like formation and propagation of cancer-cells external to the muscular coat in the areolar tissue under the peritoneum.

This formation of cancer-cells may even penetrate the walls of the bloodvessels, especially of the veins: thus, in two cases of medullary

fungus of the stomach and liver, the author found a portion of the fungus in the cavity of the adjoining vena portæ.* He could observe how, from the increased development of the cells in the wall of the vein, they had pressed into that vessel, where they were found smaller, or in other words, more recent. It is easy to understand how, under these circumstances, an infection of the blood and general spread of the disease must rapidly take place.

"It appears, however," observes the author, "that the vena portæ is peculiarly disposed to this fungous growth; I have not as yet met with it in other veins, while complete filling up of the vena portæ in the liver, even to the smaller branches, with a vast fungous mass, is not so very rare, as I can prove from many remarkable examples.

"From all that has been hitherto adduced, it is evident that when once epithelial (epithelioma) or any other species of cancer has formed, and has arrived at a certain degree of development or maturity, the extension of the disease occurs by the constant new formation of cells in the parenchymatous fluid which penetrates the adjacent parts, granulations first taking place, from which nuclei become developed, and subsequently appear to be circumvested with cells. Sometimes the number of nuclei in an already formed cell increases; these nuclei proceed to cells, and thus a parent cell arises, which is finally resolved, setting free the enclosed cells. This latter mode appears, however, to occur only in [true] cancer and medullary fungus, and not in epithelioma. We therefore assume that the parenchymatous fluid acquires the property to form similar cancer-cells, from the interchange of constituents which must necessarily take place between the cancer-cells already existing and this fluid; while the latter, gradually reaching the surrounding parts, becomes the cause of the local extension of the disease. "The adjacent parts frequently pass into cancer, but cancer-cells become developed between and in them, whereby the earlier tissues are dissolved and disappear.

"It has been supposed that the destruction or disappearance of the surrounding parts is principally owing to the pressure of the enlarging cancerous tumour. This, however, would seem not to be so exclusively the case; the morbidly altered parenchymatous fluid penetrates the adjacent textures, so that not only does a multitudinous new formation of cells take place in the areolar tissue between the other organic constituents, but the latter taking up this fluid, a similar cell-formation ensues in the tissue of the parts themselves. Thus, we saw cancer-cells form within the structureless membrane or sarcolemma covering the muscular fibres, and in like manner they seem to be developed in the cavity of the nervous tube. The formation of a quantity of fat at the same time accompanies this change, by which the healthy constituents seem to be gradually broken up; the parts are therefore destroyed, not so much in consequence of pressure, as of impeded nutrition, due to the morbid condition of the parenchymatous fluid, the development of fat, and the solution of parts which everywhere attends the latter. This diffusion of morbid parenchymatous fluid does not, however, take place solely in the immediate neighbourhood of the cancerous tumour, but the same fluid is taken up by the lymphatics, and so conveyed to the nearest glands. This is particularly proved by the examples I have quoted, where, in epithelioma of the lips, or also of the tongue, the neighbouring lymphatic glands become swollen and filled with epithelial cells. Thus, too, I have often, in medullary fungus of the liver or stomach, seen the absorbents as thick white cords, pass through the diaphragm and along the sternum, so that I could follow them into the thoracic duct, which was also swollen,

* In a note, the author calls attention to the remarkable fact, described by him so far back as 1828, that after the injection of fibrous tumour and carcinoma, only fine capillary arteries could be seen, and that no veins appear to exist in connexion with those of the general system. It would seem, he observes, that the blood in carcinoma returns to the arteries, that a new capillary net exists between the vessels of this class, such as we find between the ramificæ of the vena portæ and the venous system.

while their contents quite agreed with the cells of the medullary fungus; and in this manner the morbid parenchymatous fluid was, through the medium of the lymphatics, thrown into the blood, and so conveyed through the whole body. But it is extremely probable that this absorption does not take place exclusively by means of the lymphatics, but likewise through the capillaries and veins, which are washed by the parenchymatous fluid, between which, as is well known, such an interchange of action exists.

"When this process has worked in a sufficient degree, and has spread through the body, we have the secondary formation of cancer-cells, and, as is well known, the development of cancer in other places.

"Of this, a case of cancer of the tongue, by which the entire of the left half of the organ was destroyed, appeared to me to be a remarkable example. The patient died of a sudden hæmorrhage, in consequence of the ramine artery giving way during the act of eating, when the copious stream of blood rushing into the mouth, and backwards into the trachea, produced suffocation. An accurate examination of this specimen, which is still in my collection, showed that not only was the further course of this vessel destroyed and changed into a black cord, but that the lingual nerve, which was very much thickened at its entrance into the cancer, passed into a black slimy band, which, during the life of the patient, I had seen lying in the mouth. Examining the nerves on the right, or still apparently sound side, I found in some degree in the lingual nerve, but especially in the hypo-glossal, little inequalities in thickness, as if tubercles had been developed within the nerve. Having made a longitudinal division of the last-named nerve, I took a very thin section from its centre, and observed numerous cancer-cells grouped together, surrounded apparently with areolar tissue, which, however, I suspected to consist in great part of atrophied nervous fibrillæ.

"This formation of cancer-cells in the centre of the hypo-glossal nerve of the healthy side, can scarcely be explained by assuming direct imbibition of the morbid parenchymatous fluid. I think we must look upon it occurring in so distant a nerve as a secondary effect, and refer it to a general infection of the fluids. It appears that little liable as the nerves otherwise are to be affected by inflammation, and even suppuration of the neighbouring parts, this is not the case with cancer, which very easily produces disturbances in them. Of this I have several striking examples in my collection; in one case, the entire sciatic nerve is destroyed at its exit from the sacrum, so completely as to sever it from its inferior portion.

"It appears to me that the burning, stinging pains which exhibit themselves in the course of cancer, are the result of cancer-cells beginning to locate themselves in, and to destroy, the neighbouring nerves; so that these very pains, in my opinion, afford a proof that the formation of cancer is no longer a local disease, but that it has already commenced to spread in the adjacent parts, and that, consequently, the time for operating, with a reasonable hope of a favourable result, has probably already passed away. It is, indeed, to be lamented that the resolution to remove a cancer is so often taken when the first period, in which the operation might be performed with a well-founded expectation of a more permanent result, has already elapsed. This is in some measure the fault of the patient, who conceals the disease so long; but the surgeon is also partly to blame, who, dreading an unfavourable impression on the mind of the sufferer, does not think himself justified in proposing operation, until he is satisfied that the disease is cancer; but at such a period a lasting cure is, in many instances, scarcely to be expected. There have been so many unfortunate cases in which, in consequence of the postponement of an early operation, a fatal relapse has subsequently occurred, that I think I cannot sufficiently insist on the removal of every swelling or hardening, from which cancer might afterwards become developed, even though this should be very uncertain. The operation is then of little importance, and even if the tumour had been a benign one, is not thereby rendered more injurious.

"If we trace the formation of scirrhus and cancer, this becomes, in my opinion, still more evident. The scirrhus in the beginning forms as a hard tubercle. If

this be examined under the microscope, we find nuclei and very small cells enveloped in and surrounded by fibres."

As an example, the author gives a drawing of a scirrhus taken from the uterus, in a case in which fully-formed cancer existed in the breast, so that scarcely any doubt could arise as to the true nature of the scirrhus. Here are seen several groups of little cells and nuclei, surrounded and enclosed in a number of fibres, which latter make up the chief bulk of the scirrhus, and among which some few elastic fibres are visible. Through the intervention of fibres, the cells are beginning to be divided into secondary groups. The state of the breast is represented in another figure; the cells are here much larger, and the tendency to form secondary groups is more plainly seen. By simultaneous increase of the cells the whole fungus enlarges, while the quantity of connecting tissue, which appears not to be so rapidly reproduced, or is perhaps removed, is proportionally much less.

"It is known that the growth of scirrhus is very slow; no great interchange of matter is, therefore, as yet taking place; by the slow increase of the cells and consequent enlargement of the groups, the surrounding tissue becomes tense: hence the hardness of the tumour. No cells or nuclei as yet exist in the adjacent tissue external to these enveloping fibres; the disease may therefore be considered as a local one, which may still be advantageously removed.

"In cancer which has already formed, the development of cells is much greater, the cells are larger, they are not separated from the neighbouring parts by such thick layers of connecting tissue and fibres; the entire tumour is less hard—that is, more parenchymatous fluid is present, and this keeps up a stronger interchange of material with the existing cells, which are reproduced with proportionally greater rapidity. Finally, if the disease is luxuriant, it is soft, often receives the name of medullary fungus, and now consists almost entirely of cells, sometimes without a trace of fibres.

"From this well-known progress and development of cancer, which I have described merely for the sake of greater clearness, it is evident that it is only in the first period of scirrhus we can reasonably suppose that, from the still sparing interchange of material, the contents of the little cells shall not have been so freely transferred to the scanty parenchymatous fluid of the tumour as to have pervaded the adjacent parts. When, however, the growth of the tumour increases, this in itself is a proof of greater activity, of an increased interchange of constituents, and of the diffusion of the infected parenchymatous fluid. When, in addition, burning, shooting pains set in, we have, I am convinced, an absolute proof that the parenchymatous fluid has penetrated the nearest nerves, and produced in them a new cell-formation, with destruction of the nervous tube. In cancer itself, it is true, as a new formation, no nerves exist; the tumour itself is insensible, so that the introduction of a stilet, if the surrounding parts be avoided, is entirely unfelt by the patient.* But if the cancer be removed, it is not only most important to take away at the same time as much of the apparently sound parts surrounding the tumour as can be done without too great injury, but I reckon it most essentially necessary, after the operation is over, to examine as accurately as possible under the microscope the edges of the part removed, in order to ascertain whether granular matter, nuclei, or cells, exist in any part of the tissue. Should this be found to be the case, we must conclude that the disease has not been wholly and completely removed, and the wound being still open, a

* Schrantz makes a similar observation, but adds, "Sometimes, however, by the increase of the tumour, nerves are included. I possess a portion of the medullary fungus of a breast penetrated by a nerve, but which, on its entrance, has become thin and transparent, and appears to have lost its contents or medullary portion."

further portion should be cut off in the situation where the cancer-cells and nuclei were seen; or perhaps, what is more painful and less certain, we should endeavour to destroy the part with caustics, in order to prevent relapses, which always lead to a fatal result."

In a note to the foregoing, the author states that subsequently to making his communication to the Provincial Society of Utrecht, in 1847, 'On the Extension of Cancer-cells by Means of the Parenchymatous Fluid,' he saw that Dr. Bennett, in his excellent work 'On Cancerous and Canceroid Growths,' had come to the same conclusion; that he had found cancer-cells in the muscles situated in the neighbourhood of carcinoma, and had thence predicted the return of the disease; and that he also insisted on the necessity and possibility of instituting, at the time of operation, a microscopic examination of every suspected tissue before the edges of the wound are closed. He takes the same opportunity to recommend the application, to open and fungous cancer, of pledgets moistened with a very strong solution of iodide of potassium, which he has found to produce a gradual diminution and apparent solution of the tumour, as well as a cessation of the disagreeable smell and hæmorrhages, while the remedy seemed to have no injurious effect on the adjacent parts. In a case of cancer of the tongue, in which this organ was so much swollen as to render eating extremely difficult, he caused the patient to keep continually in his mouth a solution of from half a drachm to a drachm of the salt in an ounce of water; the result was that the fungus became softer, and separated, in a few days, the tongue returning to its normal thickness. The application did not, however, prevent relapses, and the patient subsequently died of cancer in the neck: but certainly this simple means, which caused him neither pain nor inconvenience, had prolonged his life. If the fungus be sprinkled with the salt in the form of powder, it dwindles more quickly, and passes into mortification, without injury to the other parts.

From all that he has brought forward the author draws the following conclusions:

1. Through an interchange of material, taking place between cancer-cells and intercellular fluid, the latter acquires the property of forming new nuclei and cells of a similar nature.

2. This intercellular fluid passes, along with the parenchymatous fluid pervading the sound parts, into the textures adjoining the tumour. The parenchymatous fluid thus acquires the same constituents and tendency to form similar cells, which now become developed among the healthy surrounding tissue, in the course of the areolar membrane.

3. On account of the minuteness and small number of the last-mentioned cells, their presence cannot be detected with the naked eye; so that the surrounding parts may appear to be perfectly sound, notwithstanding that they contain the germs of the advancing formation of cancer.

4. It is, therefore, of importance, in removing cancer by operation, not only to take away at the same time a large quantity of the adjacent sound parts, but also to examine the innermost sectional edges under the microscope, in order to ascertain whether any trace of cancer-cells in process of formation is to be discovered in them.

5. The existence of burning, shooting pains in carcinoma, may be

taken as a proof that the cancer-cells have reached the neighbouring nerves, and the disease can then scarcely be looked upon as a local one, in which an operation might be permanently successful.

6. By the absorption of the infected parenchymatous fluid through the lymphatics and veins, the whole body seems to become more or less tainted, so that secondary cancer ensues in distant situations, when, as is self-evident, operation can no longer be thought of.

7. This altered parenchymatous fluid penetrates the organic tissues which are washed by it, the sarcolemma of the muscular fibres, the tubes of the nerves, &c. These membranes, too, both the sarcolemma and the walls of the nervous tubes, appear to take up the altered nutritive fluid; the consequence of which is, that both within the sarcolemma and the nervous tubes, similar nuclei and cells arise, accompanied with an absorption of the muscular fibre and of the contents of the nerve, and attended with the deposition of fat, by which these parts waste and are destroyed, while the surrounding membranes (sarcolemma and walls of the nervous tubes) remain.

REVIEW X.

The Medico-Chirurgical Transactions. Second Series. Vol. XIX.
London, 1854. 8vo, pp. 264.

THE present volume of the 'Medico-Chirurgical' is rather a thin one. Its contents, however, are, for the most part, of good quality, and do not detract from the reputation the 'Transactions' have so justly earned. We proceed to give a brief account of those papers which require notice.

I. *Scrofulous Caries of the Left Astragalus. Excision: Cure: with Formation of a Fresh Joint.* By S. F. Statham, Esq.

A record of a successful case, with nothing about it to call for particular remark.

II. *Pathological Remarks on the Kind of Palpebral Tumour, usually called in England, Tarsal Tumour.* By H. Haynes Walton, Esq., F.R.C.S.

The tumour in question is described by Mr. Walton as usually solitary, immovable, hard, spherical, and well-defined; as varying in size from that of a grain of small shot to that of a pea; as limited in position to the seat of the Meibomian glands. From examinations, by Dr. Druitt, of a specimen recently removed, and by Mr. Walton of two specimens in the College of Surgeons, it appears these tumours originate in a Meibomian gland, in which sebaceous matter and epithelium have collected, and round the orifice of which, fibro-plastic matter has been effused. Mr. Walton suggests the name of Meibomian tumour, and states, that since he has adopted the above pathological view, he has ceased to attempt its removal from the interior of the eye, but now divides the lid and cyst on the outside, squeezes out the contents, and, if possible, extracts the cyst with a pair of forceps.

III. *Notice of a Case of Skin Disease, accompanied with partial Hypertrophy of the Mammary Gland.* By James Alderson, M.D., F.R.S.*

A young lady, aged 20, presented on the upper part of the left breast the following remarkable appearance. To the extent of about four inches in length, by one and a quarter in width, the skin assumed the appearance of a perfectly smooth, polished surface, of an opaque, yellowish-white colour, like polished vellum or ivory. Round the margin of this parchment-like surface was a vascular zone; but on the surface itself were no vessels and no desquamating epithelium. Below it, some of the glandular structure was enlarged, and there was a small gland in the axilla.

Various opinions were expressed as to the nature of this affection. Mr. Hodgson considered it to be allied to carcinoma; Sir Benjamin Brodie thought it might be dry gangrene; and Mr. Ure connected its appearance with deficient catamenia. Being treated with mild frictions and liquor potassæ, the tumour disappeared, without any desquamation of cuticle or destruction of the cutis. Other tumours of the same kind appeared, however, on the arm and thigh; but the progress of these is not recorded. The general health remained perfect.

IV. *Case of Mollities Ossium, preceded by Degeneration of the Muscles.* By T. K. Chambers, M.D.

A very interesting case of mollities in an unmarried woman, 26 years old, in whom, before death, spontaneous fracture occurred in many bones. The urine did not contain any of the peculiar albuminoid substances discovered by Dr. Benée Jones in a somewhat similar case. It was analysed by Dr. Beale; but as the composition in 1000 parts alone is given, no very certain inferences can be drawn from it. It appears, however, that the urea was relatively small, and the extractives large in amount; and that the earthy phosphates and the fixed alkaline salts were relatively very greatly increased.

After death, a piece of tibia examined under the microscope, was found to consist (with the exception of a thin external layer) of large fat vesicles, with various-sized dull-red spherules between them. The external layer contained small islands of opaque bone, with indistinct corpuscles; round the islands was a fibrous structure, in which were oil globules of various sizes.

A portion of rectus muscle was found totally degenerated, and consisted of little else than fat vesicles.*

V. *On the Keloid of Alibert, and on True Keloid.* By Thomas Addison, M.D.

The object of this communication is to show that the keloid of Alibert is not the disease to which the term keloid should be applied, but that there is another and totally different affection to which this term is more applicable, and which, therefore, is to be termed the "true keloid," in contradistinction to the keloid of Alibert. There appears to us something strange in thus wresting away the original term, invented by Alibert, from the disease to which he undoubtedly assigned it, and applying it to another affection, of which he had no knowledge whatever. It would

have been better, we think, to have proposed another term for the distinct and novel affection now first fully described. Having said thus much against Dr. Addison's terms, we have only to add, that we have not one word to say against his descriptions; they are clear, precise, and so concise that we will not run the risk of injuring them by attempting condensation, but will refer our readers to the original.

VI. *On the Blood and Effused Fluids in Gout, Rheumatism, and Bright's Disease.* By A. B. Garrod, M.D.

The object of the author is to indicate a very easy mode of detecting uric acid in serum; viz., by placing a little serum in a watch glass, at the bottom of which lies a fine thread, and adding acetic acid. The uric acid deposits on the thread, and is easily recognised under the microscope by the form of its crystals. The test does not indicate the presence of the acid unless this amounts to 0.025 grains in 1000 grains of serum; and as such a quantity is always abnormal, the appearance of the crystals is conclusive as to the existence of uric acid in morbid amount. In order to use this test, the serum must be fresh, for Dr. Garrod finds that the uric acid soon decomposes; and he believes there is little doubt that oxalic acid is one of the products.

The author then reports an examination of the sweat of a gouty patient, in which uric acid was vainly sought for. He then notices that he has found uric acid in pericardial and peritoneal effusions, in cases in which the blood contains an abnormal amount of this substance. Finally, he records the interesting fact, that in the fluid of a blister applied to a gouty subject, uric acid is detected by this thread experiment.

VII. *On Excision of the Knee-joint.* By G. M. Jones, Esq., M.R.C.S.E.

The writer gives a table of all the cases of excision of the knee-joint hitherto performed, 33 in number, of which 6 were by himself. Five of these operations were performed by Mr. Jones, by two lateral incisions, and a connecting transverse one carried over the centre of the patella. In the last case a longitudinal incision, four inches in length, was made on each side of the knee-joint, midway between the vasti and the flexors of the leg; the two cuts were connected by a transverse one just below the insertion of the ligamentum patellæ, the flap was reflected up, the ligamentum patellæ and patella were pulled aside by a spatula, the leg was forcibly flexed, the crucial ligaments divided, and the articulating surfaces thus exposed. Mr. Jones believes that this operation is not more dangerous to life than amputation, and the superiority of the natural over a wooden leg will scarcely be contested.

VIII. *On the Radical Cure of Reducible Femoral Hernia by a New Operation.* By T. Spencer Wells, F.R.C.S.

The new operation was devised by Professor Wutzer of Bonn. It has been performed twice by Mr. Wells, and 58 cases are referred to, in many of which it has been successful, and in only one of which did death ensue. The principle is to pass up a cylinder into the inguinal canal, pushing up,

of course, the skin before it; the instrument is fixed by a needle passed through the skin, and then pressure is exerted on the cylinder and on the internal ring on which it rests. The patient is kept quiet, to prevent excess of inflammation; the cylinder is kept applied for six, seven, or eight days, and is then removed, and the cavity is filled with lint. We refer to this interesting paper for the full details of the method.

IX. *Observations on Morbid Changes in the Mucous Membrane of the Stomach.* By Dr. Handfield Jones.

Tables are given of 100 cases of various diseases, in which the mucous membrane of the stomach was microscopically examined:

"The following deviations from the typically healthy condition are mentioned:

"1. *Nuclear masses*.—These, as I have stated, are the solitary glands, and it is doubtful what degree of their development is to be considered as surpassing the physiological limit. It seems probable, both from actual observation, and from the behaviour of the same structures in the intestines, that they may become hypertrophied, and encroach abnormally upon the proper secreting tissue. Again, it is certain that they may undergo atrophy, and thus occasion loss of substance and thinning of the mucous membrane in the spots they occupy. . . .

"2. *Diffused nuclear formation*, in extreme instances, extend uniformly throughout the mucous membrane. The nuclei are mingled with more or less granular matter, and the tubes are more or less atrophied and obscured by the interstitial deposit.

"3. *Inter-tubular fibroid formation*.—This is very commonly associated with the preceding, and consists simply in this, that the exudation in which the nuclei lie, passes into the form of a more or less fibroid, or homogeneo-fibroid stroma. In this, elongated or fibre-forming nuclei may sometimes be seen. The material is very similar to that which thickens the Glissonian sheaths in some cases of cirrhosis. In some cases a change takes place in the tubes themselves, so that they become converted into nucleated substance, similar to that which surrounds them. Their epithelial contents are changed into a granular mass, containing many more nuclei than in the healthy state, while the homogeneous wall of the tube wastes and disappears, and so the intra-tubular nucleated mass blends with the extra-tubular, and the whole mucous membrane is converted into an uniform material loaded with nuclei. In extreme cases the tubes are utterly atrophied, and the whole thickness of the mucous membrane is occupied by fibroid or granular stuff, in which some altered remnants of the tubes may be brought into view by means of acetic acid. The basement membrane of the surface is often absent in parts where there is much inter-tubular formation, and the nucleated fibroid tissue is then exposed. It may, however, have been covered in by the columnar epithelium during life.

"4. The tubes appear, in some instances, to decay spontaneously, or, at least, not from the atrophic pressure of new-formed fibroid tissue; the mucous membrane may then present a mere mass of granular and celloid debris, with interspersed fat vesicles and fatty matter.

"5. *Black pigment* may be deposited in the mucous tissue, sometimes in great quantity; it is occasionally within the tubes, more often between them. . . .

"6. *Cystic formation* is occasionally met with. . . .

"7. *Mammillation* is often seen in lesser degrees, and, not unfrequently, well marked. It affects especially the pyloric third or half of the stomach. To obtain a good view of it, or indeed not to overlook it, it may be absolutely necessary to wipe off a thickish layer of tenacious adhering mucus. It seems to be of two kinds, or to be produced in two ways. One may be called healthy, and appears to depend on some unusual contraction of the corium of the mucous membrane. . . .

The other form of mamillation is morbid, and seems to be essentially connected with fissuring of the mucous membrane, or local atrophy. . . .

"8. *Gathering up of the lower parts of the tubes in the pyloric region* so as to form a group of convolutions, something like the acini of a conglomerate gland, is often observed. It is not quite clear how the change is produced. . . .

"9. There is much difficulty in determining exactly what conditions of the *epithelium of the tubes are unhealthy*. Their contents are often of a very opaque fatty aspect, especially in their lower half; but this scarcely seems to be abnormal. In a few instances I have observed an apparently true fatty degeneration of the epithelium, the nuclei and cells being converted into shrunken fatty masses. Not unfrequently the epithelium appears more or less stunted and atrophied, or of a less soft, finely mottled aspect, and its cells look withered and shrunk. . . .

"10. *Self digestion*, in slighter degrees, is of very common occurrence, and is invariably confined to or most marked in the splenic region. . . .

"11. Small, dark red, circumscribed spots, seen on the surface of the mucous membrane, are manifestly the result of hæmorrhage, or at least of the exudation of hæmatin. The microscope shows in these parts an abundance of dark pigment granules." (pp. 92—97.)

In the 100 cases, 28 only were quite healthy. In 47, the splenic and mid regions were healthy, while the pyloric was affected. In 11 cases there was a moderate, and in 14 cases a great, amount of destruction of tubes. The male sex was apparently more liable than the female to this organic disease. Seven cases of ulceration are referred to, all in persons over 48 years of age, the average age of the whole being 59 years.

The destruction of the tubes is not a very marked sequence of drinking: in 11 immoderate drinkers, 1 stomach was healthy, 6 were tolerably so, 1 had moderate and 3 great destruction of the tubes.

It would appear that considerable wasting of the tubes may occur without any marked symptoms.

We are happy to observe that Dr. Handfield Jones is about to give further instruction on this most interesting subject, and we defer all comment for the present.

X. *A Case of Fatal Asphyxia, caused by the Detachment of a Diseased Bronchial Gland impacted in the Larynx.* By George Edwardes, F.R.C.S.

This is a very remarkable case of a bronchial gland ulcerating through the walls, and entering into the cavity of the trachea, and then being carried up to the larynx by violent exertion, causing death by suffocation. There had been no previous reason to suspect any disease whatever: there had been no cough, hoarseness, or dyspnoea.

XI. *Remarks on a Peculiar Form of Tumour of the Skin, denominated Pachydermatocele.* By Valentine Mott, M.D.

The form thus described consists in hypertrophy of the skin and of the subcutaneous tissue, which, commencing in a congenital brown spot, or mole, gradually increased in size, although in some cases the bulk was very considerable, and necessitated operation. Five cases altogether are referred to. The description given by Dr. Mott does not add anything to our previous knowledge of the subject.

XII. *Case of Distortion of the Spine.* By Thomas Hodgkin, M.D., and William Adams, F.R.C.S.

It is apparently intended that our recollection of Dr. Mantell shall not merely be that of the ardent and genial labourer in the cause of science, but also that of the unlucky individual who was himself a scientific curiosity, and carried about with him a mysterious disease which the inquisitive scalpel of his friends laid bare after death. The dry record of the spine disease narrated by Dr. Hodgkin and Mr. Adams, gains nothing from our being informed that Gideon Mantell was the subject of it, and we cannot perceive the necessity of using the name of the patient to excite our curiosity about his disease.

The condition of the spine was that of transverse rotation of the bodies of some of the lumbar vertebrae, so as to cause passage of the spinous processes to one side, and unusual prominence of the transverse processes on the other side. We refer to the paper for the full details and remarks on the affection, which are extremely instructive.

XIII. *On Gout and Rheumatism. The Differential Diagnosis, and the Nature of the so-called Rheumatic Gout.* By A. B. Garrod, M.D.

The chief points of importance in this most valuable paper are as follows:

1. In 47 instances, the serum of the blood was found to contain an abnormal amount of uric acid. No less than 45 of these persons were males. Their average age was 47 years. All these patients suffered from articular disease of that kind which is termed "Gout," by writers. In 26 of these cases, inquiry was made into the diseases of the nearest blood-relations, and in no less than 13 some close blood-relation was found to be similarly affected. In 28 cases the habits were inquired into, and no less than 21 were found to be free livers. The occupation was noted in 33 cases, and it is very remarkable that 8, or nearly 25 per cent., were workers with lead in some form or other.

2. In 35 instances the serum of the blood was found to contain no uric acid. All these patients suffered, like the former, from an articular disease, which was so well marked as to necessitate at once the diagnosis of rheumatism, as defined by writers. In almost all other particulars these patients differed from those above referred to; and the following table, given by the author, shows at a glance the peculiarities of each class:

CLASS I. *Articular Affection with Uric Acid Blood.*

The average age of patients was	47 years.
The males formed	about 95 per cent.
Hereditary predisposition was traced	in 50.0 "
Free living and drinking had existed	75.0 "
Painters or plumbers formed	24.3 "
Drink acted as the exciting cause	in 39.5 "
The great toe had been specially affected	82.9 "
No great toe affection	5.7 "
Doubtful	11.4 "
Edema noticed	68.5 "
Deposits of urate of soda	45.9 "
Acute cardiac affection	none.

CLASS II. *Articular Affection (Non-urethral) with no Uric Acid in the Blood.**

The average age	was 40 years.
The males formed	but 40.0 per cent.
Hereditary affection was traced	in 33.0 "
Cold acted as an exciting cause*	88.8 "
Edema noticed	12.9 "
Acute cardiac affection	41.9 "
Deposits of urates of soda	none.
Great toe especially affected in	none.

Dr. Garrod therefore draws the conclusion, that to the differences already described between gout and rheumatism, another must be added—viz., the presence of uric acid in the blood in the first, and its absence in the second disease. He believes, moreover, that this character may be used as a diagnostic mark in those cases which cannot be referred by other symptoms to their proper heading.

3. In 6 cases of articular affection (discharge), clearly connected with urethral disease, in 4 cases no uric acid was found.

4. In 61 other patients, with various diseases, but without articular affections, the blood was examined for uric acid. It was present in 13—viz., in 5 persons with albuminuria, in 1 with cholera, in 1 with ophthalmia, in 1 with bronchitis, in 1 with pneumonia, and in 4 with a disease not named.

In a postscript, 14 additional cases are referred to; in 4 there was an abnormal amount of uric acid and coincident gouty symptoms, in 5 there was no uric acid and coincident rheumatic symptoms, in 6 there was no uric acid and no articular affection.

We can have no difficulty, it appears to us, in accepting the author's conclusions as to the presence and absence of uric acid in the blood of gouty and rheumatic patients. Evidently, however, the uric acid, *per se*, is not the cause of the articular affection in gout, as it is stated to have been present in variable, but, in some cases, in considerable, quantity in 13 cases in which the joints were untouched. Much remains yet to be done before the genesis of gout can be explained; and we shall hope to receive from Dr. Garrod a continuation of the important researches he has commenced. We defer all comment on Dr. Garrod's opinions on the impropriety of the usual use of the term "rheumatic gout," to another opportunity.

XIV. *Case of Traumatic Aneurism of the Ophthalmic Artery.* By T. B. Curling, F.R.S.XV. *English Statistics of Hooping-Cough.* By Edward Smith, M.D., LL.B. (London).

The information contained in this paper is derived from the returns of the Registrar-General; and is, therefore, based only on fatal cases.

In the London district only 6 diseases produce a greater mortality than hooping-cough—viz., phthisis, pneumonia, bronchitis, typhus, convulsions, and scarlatina, in the order here given. In ten years, out of 553,694 deaths, phthisis killed 68,204, and hooping-cough 18,666 persons. The

* And alcoholic fluid did not appear to be either a predisposing or exciting cause.

mortality of hooping-cough was to all diseases as 1 to 29.6. More than $\frac{2}{3}$ ths of the whole number of deaths were in children under 1 year old; $\frac{3}{4}$ ths of the children were under 2 years; and $\frac{1}{2}$ ths were under 5 years of age.

Hooping-cough is much more fatal in female than in male children, and the relative mortality increases with age.

"Thus, whilst under one year of age, the excess in the ratio of mortality amongst females is one-sixth, it is less than one-third in the fifth year of existence, and was reduced to one-fourth in the second year, and one-fifth in the succeeding intervals. It is unsatisfactory to pursue the comparison at later periods of life, on account of the smallness of the numbers to be contrasted, but so far as this is of value, it proves that this preponderance is maintained, and even increased at puberty, and for an indefinite period beyond that era." (pp. 239-40.)

The mortality and the temperature of the air are in an inverse ratio to each other, or nearly so. The following sentence gives the pith of an interesting inquiry which is made into this point:

"The mortality of hooping-cough attends diminished temperature with considerable precision, and so far may have a point of correspondence with other seasonal affections; but there is one point in which it differs from others—viz., that it is not increased in intensity by any intensity of the opposite season, or that of summer. Excessively high temperature, so far from having given rise to increase of mortality, was directly the reverse." (pp. 245-46.)

The author enters incidentally into the relative mortality of hooping-cough and some other diseases, and gives a diagram, showing the mortality of bronchitis, of hooping-cough, and the temperature, which is exceedingly interesting. It clearly appears that the mortality of these diseases bears a very close relation, and depends to a very great extent on temperature, being greatest when this is least.

We strongly recommend this paper as an excellent example of statistical inquiry.

Having now completed the notice of this volume of the 'Transactions,' we shall only add, that though small in bulk, its papers are, in many cases, of high merit. It is much better for the Society who print, and for the public who read books, that there should be careful winnowing, otherwise the Society loses caste, and the public gains only a mass of unreadable and useless dissertations.

REVIEW XI.

On Pain after Food: its Causes and Treatment. By EDWARD BALLARD, M.D. Lond.—London, 1854. 8vo, pp. 136.

DR. BALLARD has done himself injustice in the selection he has made of the title for this work; it reads somewhat *ad captandum*, and does not, without due reflection, convey the full extent of its comprehensiveness. The author has, in truth, composed a treatise upon indigestion. Pain after food is but one among many of the symptoms and effects thereof, which he treats of. The arrangement of the work is novel. We have first an "Introduction," which discusses the physiology of digestion so

far as the stomach is concerned. We next meet with "The Symptom," pain after food, and its accompaniments. The seat, character, intensity, and variety of the pain; its dependence upon the quantity and quality of food, are all considered at length by the author; together with its attendants, eructation, vomiting, thirst, feverishness, &c. The author, in the third place, gives a tabulated arrangement of the causes and treatment of pain after food, with numerical references to the paragraphs which precede and follow it. In this table consists the novelty and ingenuity of the work. As reference is made in every section of it to other parts of the work, so that it is a kind of directory index to the whole; the "Commentary" which follows necessarily recapitulates the matter of the previous chapters, "Introductory" and on "The Symptom."

The treatment, dietetic and medical, laid down by the author, is in all cases scientific and judicious.

A few quotations will show the nature of the work and its value. With respect to the seat of pain after food, Dr. Ballard informs us that—

"The most frequent *seat of the pain* is the epigastrium or lower half of the sternum and neighbourhood of the ensiform cartilage. It is thus referred in considerably more than half the cases which are met with in practice: sometimes the seat of pain is so circumscribed that it may be covered with the point of the finger. The next most frequent seats of pain are the region of the umbilicus, and the entire upper portion of the abdomen, stretching from one hypochondrium across the epigastrium to the other. In some, it is referred to the lower part of the interscapular region, and in others to the situation of the heart or to a spot below the left mamma, and near the situation of beat of the apex of the heart; in a few, to the lower region of the abdomen or hypogastrium." (p. 11.)

The character of the pain is variously described, as—

"Weight," 'oppression,' 'tightness,' 'fulness,' or 'tension,' and all these terms, imply a *dull* kind of pain and uneasiness. The patient sometimes uses some special simile to illustrate his meaning, and the pain is most frequently compared to 'a cord drawn tightly round the body,' to 'a heavy load lying' upon the part or internally, or to a sensation of 'being blown up' with flatus. Other kinds of dull pain are designated as 'aching,' occasionally as 'throbbing,' and sometimes as 'sinking.' Next to the dull pains, in order of frequency, come those which are described as 'spasmodic,' 'twisting,' 'pinching,' 'tearing,' 'dragging,' 'gnawing,' and 'scraping,' all of which, with the 'darting or lancinating pain,' 'like a knife or sword running through,' are to be enumerated under the head of *acute* pain. . . .

"The several varieties of pain described are not confined each to its own locality; but yet it is possible, in a general way, to make some sort of *topographical distribution* of them. Thus the dull kinds of pain are, with few exceptions, referred to the sternum, epigastrium, ensiform cartilage, interscapular region, or upper part of the abdomen, rarely to the hypogastrium or inferior regions. The more acute kinds of pain are mostly located at the seat of palpable tumours, at the ensiform cartilage, the region of the heart, the iliac regions, the parts about the umbilicus, the lower part of the abdomen, or the abdomen generally. Those pains which patients describe as 'gnawing,' or 'scraping,' affect the epigastrium, and lower end of the sternum, and neighbourhood of the ensiform cartilage, much more frequently than any other part; while those described as 'soreness,' 'smarting,' or 'burning,' are most frequent at the epigastrium, lower part of the sternum, and upper part of the abdomen generally, sometimes also being referred to the situation of a palpable tumour." (pp. 12—15.)

The pain is connected frequently with the quantity, less commonly with the quality, of the food which has been taken; the quantity, tempe-

nature, and quality have variable effects. The time at which pain occurs after food is variable, from a minute or two to two or three hours subsequently. Its duration is equally uncertain. It is often associated with the following symptoms—tenderness on pressure, thirst, feverishness, vomiting, eructation of liquids or gas, borborygmi, or temporary abdominal swelling, from development of gas. The causes of pain after food, and the remedial measures necessary in each case, are then given in the table before referred to. We throw the former into the following scheme:

Causes of Pain after Food.

I. Unusual irritation	A. Irritating ingesta....	Temperature too high or too low. Alcoholic liquids. Stimulant condiments.
		Food too hard originally or from preparation. Food badly masticated from various causes. Food imperfectly prepared by insalivation from various causes. Gastric juice deficient from too great, too rapid, and too frequently repeated eating; from mental distraction; from bodily fatigue; from atony, anaemia, hyperaemia, or structural changes of the mucous membrane; from flatulent distension; too great dilution of food. Muscular movements of stomach deficient from atony or hyperaemia.
	B. Ingesta irritating from delay in the stomach	
	C. Ingesta not properly carried from the stomach	In atony, paralysis, organic disease, mechanical obstruction.
	D. Over-acid secretion into the stomach	From various causes of irritation, organic disease; gout, diabetes, blood disease, pregnancy, &c.
II. Abnormal sensitiveness of the stomach*	E. Acid changes in the food	From delay, or from bad secretion of gastric juice.
	A. Exalted nervous impressibility	General. Local.
III. Perforation of the stomach.	B. Hyperaesthesia from hyperaemia and inflammation	From prolonged irritation, from intropulsion from surface, retrocession of cutaneous eruptions, febrile excitement, impediment to portal circulation, from suppressed hæmorrhages, irritant poisoning; cancer, chronic ulcer.
IV. Distension of the stomach	A. By excess of food. B. By flatus.	
V. Spasm of stomach.		
VI. Abnormal conditions external to the stomach	A. Contraction of the space which is normally allotted to the variations in size of the stomach.	
	B. Diseases in which the contractions of the intestines are painful.	
	C. Diseases of abdominal organs accompanied with tenderness.	

We have somewhat shortened Dr. Ballard's arrangement, and have not included all his minor points. We question whether he has not been unnecessarily minute, and whether, without any real sacrifice of accuracy, he might not have avoided repetition. However, the table is decidedly an useful one.

To every heading is affixed a number, which refers to a paragraph in the "Commentary" following the table, and composing the bulk of the work. We shall refer to one or two points.

The fourth cause of "unusual irritation" is stated to be over-acid secretion into the stomach. We turned with some curiosity to the paragraph in the "Commentary" in which this point is debated, to see if Dr. Ballard had any real evidence to offer of increased acid secretion, apart from acid production, by changes in the food. He accepts, however, without question, the assertion that the gastric juice may be more acid than natural, without referring to his authorities. The production of acid in the food, lactic, acetic, and sometimes hydrochloric (possibly from decomposition of the chloride of sodium), can be proved with tolerable certainty; and it is also likely that the secretion of acid may, as Dr. Ballard asserts, be too abundant. But as such evidence on this point as is known to us is very inconclusive, we should have been glad to know Dr. Ballard's precise grounds for his belief.

Under the head of flatulence, again, we have the common and obvious cause of changes in the food; but in addition, Dr. Ballard speaks of a secretion of gas from the mucous membrane. In proof of this, he refers in the "Commentary" to those cases, in aged and in hysterical persons, in which the stomach gets sometimes filled with air with great rapidity. But hysterical women certainly swallow vast quantities of air; and when, as in some cases, the distension of the stomach and intestines is so rapid as to lead us to suppose it must arise from actual secretion of gas, there is, it appears to us, usually no pain.

* Under the head of atony, Dr. Ballard writes: "This state of the stomach may be, and commonly is, associated with general muscular debility or atony, the recognition of the presence of which, and of its known causes, is consequently most important in the diagnosis of the pain after food. When there is general atony, its signs are exhibited throughout the muscular system, voluntary and involuntary."

But surely there are many cases of general paralysis with atrophy of almost all the voluntary muscles, and many cases of fatty heart without the least coincident affection of the stomach. Do these affections run at all parallel? When atony of the stomach occurs without this general affection, by what signs can it be recognised? Are they entirely negative or positive signs?

We should have been glad to have seen these and other topics more fully treated of in the "Commentary"; and if Dr. Ballard had referred to his authorities, he might have written a longer and a heavier work, but it would have been one of greater permanent value. The work is, however, a very useful one; and all who have to treat the troublesome symptom it discusses, will find it an useful guide in discovering the causes, and in suggesting the remedies, of pain after food.

REVIEW XII.

1. *A View of the Formation, Discipline, and Economy of Armies.* By the late ROBERT JACKSON, M.D., Inspector-General of Army Hospitals. Third Edition.—London, 1845. 8vo.
2. *The Queen's Regulations for Army Hospitals.*—London (unpublished). 8vo.
3. *The Medical Codes of the Bengal, Madras, and Bombay Armies.* Fort William, Fort St. George, and Fort St. David (various dates). 8vo.
4. *Etudes sur le Service de Santé Militaire en France, son passé, son présent, son avenir.* Par L. J. BÉGIN, Chirurgien-Inspecteur, &c.—Paris, 1849. 8vo.
Remarks on the Military Medical Service in France; its Past, its Present, and its Future. By M. L. J. BÉGIN.
5. *Réorganisation du Service Sanitaire de l'Armée Belge.*—Bruxelles, 1847. 8vo.
Reorganization of the Sanitary Service of the Belgian Army. Brussels. 1847. 8vo.
6. *Letter to the Right Hon. the Secretary at War on the Medical Department of the Army.* From SIR GEORGE BALLINGALL, Regius Professor of Military Surgery in the University of Edinburgh.
7. *Recueil de Mémoires de Médecine, de Chirurgie, et de Pharmacie Militaires redigé, sous la Surveillance du Conseil de Santé.* Par MM. JACOB, Docteur-en-Médecine, Ancien Pharmacien-Major; PARDIN, Médecin-en-Chef de l'Hôpital Militaire du Roule; et par interim A. JUDAS, Ancien Médecin Principal de Première Classe. Publié par ordre du Ministre de la Guerre. Deuxième Série. Troisième Volume.—Paris, 1854. 8vo.
Collection of Memoirs of Military Medicine, Surgery, and Pharmacy, drawn up under the Superintendence of the Council of Health.

FEW subjects connected with the war now waging with the greatest and most aggressive military power of modern times, have excited so intense and painful an interest in the public mind, as the state of the sick and wounded of the British army in the Crimea.

The medical department has been charged with the gravest sins of omission and commission. To its alleged inefficiency, many have not scrupled to attribute much of the deplorable sickness and suffering, which have called forth the active and generous sympathy of the entire nation.

One of the many features which characterize the contest to which the Cross is now committed in defence of the Crescent, is the marvellous rapidity with which the minutest details of operations destined to occupy so engrossing a page in history, are circulated to every hearth and home in the kingdom. They are typical of our times, and are to history what the photographic representations of evanescent scenes are to the pictorial art. The magic process by which the fleeting foam of the curling wave is stamped upon the fairy film, and caught ere its sparkling drops are

absorbed in the great ocean from which it for an instant rose, has been imitated in the records which daily and hourly reach us from the seat of war. The magnificent word-pictures of the many able correspondents present with the field force—than which the narrative of no human events contains matter more full of vivid and undying interest—have enabled us to realize the scenes of brilliant valour and patient suffering now enacting in the Crimea, with a clearness of which the published records of the preceding campaign afford a trace. The imperishable feat of arms which concluded the great contest in which our fathers were engaged, could not have more completely riveted the attention of the civilized world, than does the tremendous struggle in which the Western Powers are now engaged, to roll back once more the tide of Northern barbarism, which, again, after the lapse of so many centuries, threatens to Vandalize the fairest portions of the habitable globe.

The busy note of preparation, heard faintly and fitfully at Varna but a short while since; the sailing of the mighty armada, to which the epithet of grand must henceforth be transferred; the heroic rallying of its gallant host from the blighting influence of pestilence and death in the forms most abhorrent to the nature of the warrior; the bold and bloodless triumph of the unopposed descent upon the Crimea; the brilliant victory of the Alma; the masterly, but probably mistaken, movement on Balaklava; the wondrous tilting of the British cavalry, paling the romance and heroism of chivalry itself; and the crowning contest on the heights of Inkermann, to be ranked hereafter with Thermopylæ and Agincourt, are already, on the threshold of the contest, indelibly graven on the tablets of time. Not less striking and memorable has been the marvellous *morale* which has characterized this immortal band of islanders in sufferings and privations equalled only by those of the retreat from Moscow, or the disastrous destruction of the Cabul force. Any other army in the world would have succumbed, utterly demoralized, in such apparently hopeless circumstances; and yet, there was not a moment when a single soldier in that force capable of raising a firelock or wielding a sword, was not ready to meet and vanquish any human foe who dared to assail him in the broad face of day, or steal upon him exhausted by a night of vigil and fasting in the trenches.

That such men should have been sacrificed to a defective system of organization, and the absence of prudence and forethought, can now be only a source of profound and unavailing regret. It was believed, that the genius of the Great Duke had raised the military character of the nation to a standard that secured it for all time against the recurrence of Walcheren expeditions, and the failures that earned for Great Britain the contempt of Europe in the times of the First and Second George. The correspondence of that eminent soldier, as contained in the record given to the world with his sanction, has shown how he acted in circumstances of peril and privation, the means which he adopted to rid his army of official incapacity, and the eminent success which crowned his efforts. The world was not prepared to find the pupils of that great master in the art of war so oblivious of his maxims, as to commit, without the faintest shadow of excuse, the very faults which he so strongly reprobated. The deeply humiliating spectacle has been exhibited of a British

army, unequalled in daring and discipline, perishing of want in the vicinity of abundance; naked and tattered within sight of stores of clothing; clothed, fed, and transported when sick, by a gallant and generous ally, to whose superior organization it owes its very existence.

The department upon which it has been attempted to cast the greatest amount of obloquy has, in truth, been the least deserving of censure. In spite of deficiencies which the parsimonious paring of peace had produced, to an extent that would have paralysed any other branch of the service, the labours of the medical officers have been more successful than those of any others of their fellow-sufferers in the camp before Sebastopol.

The skill of the engineers, aided by the best practice of the artillery, has failed, as yet, to produce any impression upon the stronghold of Russian power in the Black Sea. The gallant efforts of the fleets have been equally impotent against the seaward defences. Such has not been the result of the surgical labours consequent on the great battles fought, or the medical skill which, in the transport of more than twelve thousand sick from Balaklava to Scutari, has kept the mortality down to six per cent. Whenever professional knowledge could be applied with any reasonable chance of benefit, it has been granted cheerfully, willingly, and successfully; and the practice of the medical officers in the field will bear the strictest comparison with that of the best regulated and most efficient hospitals in any capital of Europe.

Such being, as we conscientiously believe, the case, we purpose to consider the shortcomings with which the medical department has been charged, the causes to which they are really due, the means by which they may be avoided for the future, the organization of the medical corps generally, and the measures required to place it upon the footing of complete efficiency demanded by the exigencies of the great contest in which it must necessarily take so prominent a part.

The present appears also to be a favourable opportunity of bringing to the notice of the profession the medical arrangements of the Indian army, by which provision is made for the care and treatment of a force of nearly 400,000 fighting men, as well as the introduction to a population exceeding that of the whole of Europe, of the art and science of medicine as now taught and practised in the West.

With regard to the alleged deficiency of medical officers in the Crimea, it is easy to prove that the presiding authorities of the department exerted themselves to meet the demands of the campaign with an energy that cannot be too highly commended. Had a tithe of the foresight exercised by the Director-General been exhibited by the heads of other departments, the world would not have been scandalized by the miserable picture of suffering daily exhibited in the public journals; and the reported capture of Sebastopol would have been the greatest fact, instead of the greatest fiction, of the past year.

The peace establishment of the medical department was, if possible, less calculated to meet the urgent demands of actual warfare, than any other branch of the army. *It had no purveyors, no hospital establishments deserving of the name, no apothecaries, and a complement of commissioned*

officers barely adequate to discharge the duties connected with the sick in garrison, in various parts of the world.

So rigidly was the number reduced to the minimum sanctioned by unwise economy, that medical officers were seldom able to leave their posts until worn out, and not unfrequently destroyed, because they could not be relieved from duties which humanity forbade them to abandon. When compelled themselves by sickness to visit Europe from distant and unhealthy colonies, if unable to return to active duty within a brief period, they were forced upon half-pay, because the regulated establishment could not be exceeded, and their places *must* be supplied.

Here, as elsewhere, there was no reserve to provide against contingencies inseparable from service in the tropics, or other places unsuited for the unacclimated European. The 21st Fusiliers in Bengal, in the early part of 1840, was not only without a single medical officer of its own, but was under the charge of a Company's assistant-surgeon of three months' standing, with two officers junior to himself in the same service, to aid him in the medical care of the regiment. Not an officer in the royal army in that Presidency could be spared for so important a charge, while a large portion of the corps was in hospital from cholera, dysentery, and fever. Until very recently, there was scarcely a Queen's regiment in India that was not indebted to the Company's army for one or more of its assistant-surgeons; and, even now, they are occasionally to be found performing duties for which the complement allowed by the Crown is insufficient. The hardship of punishing medical officers for afflictions too often caused by the arduous nature of their duties, is confined to that branch of the army. The sick engineer is not placed upon half-pay until he can recover his health; nor does any other staff officer forfeit his position in the active branch to which he belongs, in similar circumstances.

The demand for an increased medical establishment came upon the country almost simultaneously with a severe epidemic visitation of the most formidable of modern scourges, to combat which the entire available force of the faculty was barely sufficient.

Very many of those, whose standing in the profession rendered them the most desirable recruits, were absent, scattered over the vast surface navigated by the merchant navy of Great Britain. Some time, of necessity, elapsed ere their services gradually became available. There was no medical militia to fall back upon, to recruit the line of the profession. And yet, in spite of all these untoward circumstances, a larger number of medical officers was sent to the army in the East, than had ever before accompanied a British force to the field.

That they have proved numerically insufficient for the wants of that army, can scarcely be with reason attributed to any neglect of the medical authorities. The inducements to enter the service at all, are not sufficient to tempt the largest and best class of medical men to seek military employment. This is a matter for the State to rectify, and the sooner it is done, the better will it be for the army, and for the national credit.

The medical staff of the expedition sent out since the spring of 1854, amounts to 273, distributed as follows:—2 inspector-generals, 6 deputy-inspectors, 18 staff surgeons of the first class, 29 staff surgeons of the

second class, 110 staff assistant-surgeons. The remainder includes the regimental complement of a surgeon and an assistant to each corps of cavalry, a surgeon and three assistants to each regiment of infantry, and a surgeon and sixteen assistants for the artillery.*

The increment of an additional assistant to each regiment of foot was an unwise measure, as subsequent experience has proved, and as Dr. Smith originally pointed out. The sick and wounded of regiments in the field are most judiciously sent, as early as practicable, to field hospitals, and thence transferred to Scutari, where provision can be made for their careful and efficient treatment to an extent that is neither desirable nor possible with their own corps. These, whether engaged in the arduous duties of the trenches, in defending the outposts of the army, or otherwise employed in active operations against the enemy, could not be hampered with the helpless and disabled, without serious detriment to their own efficiency, and readiness to encounter the ever-varying incidents of a strenuous struggle with a highly-disciplined, watchful, and wary foe. The regimental surgeons have not, therefore, the same fixed hospitals as in time of peace, and except for the first few hours or days succeeding an assault or a general action, have not the same continuous demands upon their time and attention as the staff surgeons. The latter required strengthening to an extent proportioned to the casualties of the campaign, whether medical or surgical in their nature.

— By the Royal Warrant of October 1st, 1840, the medical hierarchy of the Queen's army consists of—

Assistant-surgeons, ranking with lieutenants.

Regimental surgeons, and staff surgeons of the 2nd class, with the relative rank of captain.

Staff surgeons of the 1st class, equivalent to majors.

Deputy inspector-generals of hospitals, corresponding to lieutenant-colonels.

Inspector-generals of hospitals, with the grade of colonels.

A director-general to rule over the whole, with the rank of brigadier-general.

The accompanying tabular statement exhibits the full pay of the various officers mentioned, with the exception of the head of the department.

FULL PAY.

Rates of daily pay, subject to the provisions of Warrant of 14 Oct. 1840.

Rank.	After 25 years' actual service.	After 20, but under 25 years' actual service.	After 10, but under 20 years' actual service.	Under 10 years' actual service.
Assistant-surgeon	£0 10 0	£0 10 0	£0 10 0	£0 7 6
Regimental surgeon and staff surgeon, second class	1 2 0	0 19 0	0 15 0	0 13 0
Staff surgeon, first class	1 4 0	1 2 0	0 19 0	—
Deputy inspector-general of hospitals	1 10 0	1 8 0	1 4 0	—
Inspector-general of hospitals	2 0 0	1 18 0	1 10 0	—

* The pay of the director-general* is 1200*l.* annually, a sum utterly in-

* This statement is compiled from the Monthly and Quarterly Army Lists to the 1st of February, 1855.

adequate for the chief of such a department, and less than is earned by many general practitioners in the city of London.

In this, as in almost every other regulation relating to the medical department, it is treated with an illiberality discreditable to the country, and little calculated to secure the services of men of ability. The pay of a staff surgeon of the first class is barely sufficient to enable him to live in a manner consistent with his social position, and in the matter of horse allowance he and all his professional staff brethren are not placed in the same position as other staff officers.

It would be tedious to refer to all the regulations on the subject in proof of the injustice pointed out; it will be easy to do so, if the accuracy of the statement is impugned.*

The nominal rank of a medical officer is a mere sham, as it is invariably set aside whenever he comes in contact with his purely military brethren, even when no question of military command is concerned.

It is but a short while since he was placed on the footing of a combatant, although fully exposed to all the vicissitudes, and many of the casualties of war. Unlike his brethren in arms, there is no peace for him. He is perpetually engaged in warfare with disease, and more exposed to suffer in health and life from contact with contagion, than any other class of military men. He enters the plague ward or the focus of fever with a greater amount of heroism than animates the soldier in the deadly breach or the daring charge. There is none of the pomp and circumstance of war, with its glare and glitter, to urge him to face the grim enemy, and to crown him with the laurel of the victor amidst the shouts of his admiring comrades; but the moral courage, which the great Napoleon held to be double that of the physical impulse which urges men to deeds of daring, never fails him, or allows him to shrink from the performance of any duty, however perilous.

On the field of battle and in the presence of the enemy he is frequently exposed to the missiles dealing destruction around him, and "confronts death without seeking to inflict it." And yet, the honours grudgingly bestowed are as sparingly awarded to him, as if he incurred no risk and had done nothing to deserve well of his country. He serves, indeed, "under the cold shade of aristocracy," and has little more to sustain him than the consciousness of performing, fearlessly and faithfully, the highest and most responsible duties that fall to the lot of man. Mercy and humanity are his mission. To give sight to the blind, strength to the weak, assuaging of pain to the anguished, relief to all whom human aid can rescue from the grasp of the fell destroyer, are his daily task. "*Nullâ re homines propius ad Deos accedunt, quam salutem hominibus daudo,*" said a wiser and a better man than any living War Minister, who damns the department with faint praise, and mentions the heroism of one of its members, whose life was sacrificed on the altar of duty, in an apologetic strain—as if apology were well-timed or necessary to introduce a name to

* The most unjust and detrimental of all are the fact of medical officers not drawing the pay of the higher rank to which they are promoted for some time—occasionally, years—afterwards, and the grossly illiberal manner in which acting assistant-surgeons are treated. As there are no acting ensigns and lieutenants, so there should be no acting assistant-surgeons. Every encouragement should, on the other hand, be held out to induce men to serve during the war, who may not wish permanently to adopt the army as a profession.

the courtly peers that is worthy of being recorded with those of Eldred Pottinger, and the gallant defenders of Silistria. In real heroism, the exertions of Dr. Thomson upon the cold and silent field of the Alma, exposed at every moment to receive death from the hands of those for whom he remained to preserve life, probably exceeded the chivalrous defence of Herat, and the wondrous preservation of Silistria against the prowess and skill of the best soldiers in the Russian camp. A courtly eulogy is all the reward likely to be bestowed upon the memory of one, whose rare virtue is deserving of a nation's gratitude!

In the French army, which there can now be no excuse for rejecting as a model in this and many other military matters, the Legion of Honour is thrown open to medical officers on the same conditions as to other branches of the army. Distinguished merit can secure it in all ranks, and the highest position in the hierarchy of decoration can be obtained by the military surgeon.

In the first and only distribution of the honours of the Bath yet made to the medical officers of the British armies, the second class was deemed sufficient for the director-general, and for one officer of the Indian army. It would not be difficult to discover in the list of Grand Crosses men without a title of the claims of Sir James Macgrigor to be placed there—general officers who have lost the baggage of their armies, gained doubtful victories, and failed in every important enterprise entrusted to them.

~~The fountain of honour appears to have dried up, after its first great effort to sprinkle incense upon the martial sons of Æsculapius.~~

In the recent Burmese campaign a field officer was made a Companion of the Bath who was never under fire during that passage of arms, and the chief of the medical staff was, as usual, left undecorated.

In the small Belgian army, when it was reorganized in 1847, there were medical officers decorated with the orders of Leopold, the Cross of Iron, and the Legion of Honour; and among them was one assistant-surgeon.

There are peace services as well as war services, and in many instances the eminence attained in the former is of a much higher order than can ever be acquired in the latter. All are alike unrequited and disregarded.

It may be urged, that a craving for earthly honours and distinctions is unbecoming a profession whose highest reward is the consciousness of doing good. It would be easy to expose the fallacy of such arguments, if they were deserving of consideration. So long as rank, and titles, and other badges of honour, are deemed marks of the approval of our fellow men, none need be ashamed of desiring to be classed among the deserving.

A large portion of popular indignation has been expended upon the reported deficiencies of lint, medicines, medical comforts, and even the most simple and necessary means of treating the sick and wounded. Subsequent inquiry, and the testimony of those best entitled to belief in such matters, has shown that ample provision for ten thousand sick and wounded was made by the director general, as soon as the nature and extent of the proposed campaign was made known to him. Beds, bedding, lint, bandages, instruments, and physic, were provided upon the most liberal estimates furnished by past experience, for the number above mentioned.

That number has not yet been attained,* and never would have been approached if proper means had been taken to clothe, feed, and lodge the soldier, so as to protect him from all avoidable causes of disease and disaster.

In spite of the most urgent entreaties, remonstrances, and representations, a vast *depôt* of hospital stores was left at Varna, within forty-eight hours' sail of Scutari, while the sick and wounded at the latter place were undergoing much unnecessary suffering and mortality from their absence. The noble and unparalleled sympathy of a generous nation was roused, and a profusion of supplies of all descriptions found their way to Constantinople, and were available at Scutari, for many weeks before the short distance to Varna could be accomplished by the culpable and incapable authorities whose duty was so shamefully neglected.

The medical authorities furnished the means and appliances. *To convey them to the scene of action; to see that they were carefully and properly packed; to regulate their issue at the proper time and place—was absolutely beyond their control.* To hold them responsible for the result is about as reasonable as to charge them with the surprise at Inkermann, the sacrifice of the light cavalry at Balaklava, or any other of the acts of neglect and incapacity, which have paralyzed and nearly destroyed the field force in the Crimea.

Not only have the medical officers no authority in such matters, but their representations regarding the sick are said to have been disregarded, unacknowledged, and treated in a manner as unbecoming as it has proved prejudicial to the service. In one recent example, it has been publicly stated that a staff surgeon of the first class was threatened, by no less a person than Lord Raglan himself, for telling the truth in rather plainer terms than was palatable at head-quarters. Had his statements been false or exaggerated, the means of disproving them were easy and at hand. There is, in sad and sober truth, no circumstance connected with the disgraceful disorganization of the army in the East more discreditable, and less characterized by magnanimity, than the treatment of the medical department of that force. It is to be hoped that the public will be fully informed upon this matter, when the result of the Commission sent out by Dr. Smith shall have been made known.

The absence of the ambulances at the battle of the Alma, and their subsequent inutility, have also been, with equal injustice, charged against the medical authorities. The organization of that corps was the result of no medical mismanagement, for the selection of the men was made without the knowledge, approval, sanction, or official cognizance of any professional authority whatever.† Indeed, it is absolutely impossible that the junior assistant-surgeon in the service could have been guilty of the grievous ignorance of selecting a body of men past the period of active life, for such duties as are required in an ambulance corps. This body, such as it was, was brought down to the beach at Varna, and, after much difficulty on the part of the military authorities, actually embarked, when, at the eleventh

* February 1st, 1855.

† We have, since the above was written, seen a statement to the effect that it was organized by Colonel Tulloch and Mr. Guthrie.

hour, it was again disembarked and left behind. *It was displaced to make room for a troop of Dragoons, which was, after all, never taken on board; thus forming an additional link in the unparalleled chain of blunders that will immortalize this expedition. The medical authorities were in no wise to blame in the matter.*

The state of the hospital at Scutari has, by those who ought to have known better, been attributed to the neglect of the medical department. That much confusion, and a proportionate degree of personal suffering, will, in all cases, result from the sudden influx of an extraordinary number of sick into even a well-appointed civil hospital, is undoubted; but that it should therefore be regarded as a proof of the neglect or incompetence of the staff of that hospital, is by no means a logical or necessary inference. It has already been shown that the apparatus necessary for furnishing such a hospital was lying at Varna, beyond the reach of the medical officers. Any one at all acquainted with the necessary and complicated machinery which an army surgeon requires to set in movement to procure what should be under the immediate control of his own department, will at once understand the difficulties that must have been encountered at Scutari to procure even what was procurable upon an emergency that would not have existed but for the unpardonable neglect of those whose duty it was to transfer the invaluable resources rotting at Varna to Scutari, as soon as the campaign in the Crimea was determined on. The temporary expedients resorted to in all great cities during severe epidemic visitations of disease, when the ordinary hospital accommodation is unequal to the emergency, shows how great are the difficulties inseparable from such calamities, in the heart of civilization and abundance. All have borne testimony to the unwearied exertions of the surgeons, many of whom, from over-fatigue and anxiety, were fitter inmates for the wards than for the ranks of active duty. Nor can language convey too exalted an estimate of the invaluable labours of Miss Nightingale and her band of nurses. The employment of female agency in alleviating the horrors of war and bringing the most sacred of all sources of consolation to the pillow of the dying soldier, can only be fully appreciated by those familiar with such scenes.

Much stress has been laid upon the unnecessary exposure, suffering, and consequent mortality in the sick and wounded transported from Balaklava to Scutari. The returns recently published in one of the medical journals show that the number of deaths was grossly exaggerated; and although that document is incomplete in not showing the actual number disembarked after detention upon the completion of the transit, it proves that considerable success attended the exertions of the surgeons who were sent in charge of those freights of human misery.

It would, in the most promising circumstances, be almost impossible to exaggerate the horrors and sufferings of a sea passage, during the winter, for the sick and wounded. The angry Euxine is not very favourable for such expeditions; nor are the closure of ports and battening down of hatches calculated to mend matters. *But the fitting-up of transports for the sick is to a very small extent under medical management, and the*

surgeon is compelled to take what is provided for him, however strongly he may disapprove, and vainly he may protest against it.

Having thus considered, however imperfectly, the shortcomings with which the department has been charged, let us for a moment turn to the causes to which they may fairly be traced.

The first and greatest of all has already, in some measure, been discussed. It is the utter inadequacy of the peace establishment to meet the emergencies of war.

The next in importance is the unwise neglect with which the counsels of medical officers of all ranks are treated, due probably, in some degree, to the improper position occupied by them in our military system. This is to some extent shared by the medical branches of all the armies in Europe, but in none is it carried to so injurious an extent as in that of England. This point is so important that we must stop to consider it.

It would not be difficult to prove that the medical department is by far the most highly educated and scientific corps of the service. The only one that can be compared with it is the engineer department, and even this does not embrace in its duties and acquirements the extent and variety of knowledge required and possessed by a well-educated member of the medical profession.

The young surgeon, after undergoing the usual school training in the ordinary classical and general education afforded by our public ~~universities~~ commences his professional training at the age of sixteen. This extends over a period of five years, embracing the study of anatomy, physiology, chemistry, botany, natural history, and the purely professional subjects of surgery, physic, midwifery, and forensic medicine. To this is superadded the practical study of disease in hospitals. To obtain such an acquaintance with the subjects above mentioned as shall qualify him for the practice of his profession, demands considerable mental powers, unwearied application, and great self-denial. All graduates in medicine who have received a liberal education are usually acquainted with one or more modern languages, and are generally as well-informed as most members of the other learned professions in the departments of literature and science that form parts of university schemes of study.

The young engineer starts from school with perhaps as good a preliminary preparation, but his professional course of study cannot for a moment be compared in extent, variety, and importance, either as a mental training or as a special qualification, with that of the cultivated physician.

Nor can his subsequent duties be deemed more useful, or of a higher order, than those of the medical calling. Road-making, bridge-building, sapping and mining, fortification, the attack and defence of fortified places, geological surveys, and the various other functions that fall to the lot of the engineer, demand no higher powers of mind, no greater moral and physical courage, no larger development of intellectual resources, than do the duties of the medical officers in the camp and the field. The soldier, to fight, must be possessed of health and strength. The destructive agencies banded together against his life and limb are infinite in actual warfare, and the highest resources of the healing art are never more sorely and severely taxed, than in diminishing or neutralizing those agencies. We have no hesitation in declaring our belief—and the history

of all contests proves the accuracy of the deduction—that *the perfect organization of the medical arrangements is the most important element of success in war*. If they fail, and an army melts away from disease, the most brilliant valour, the most skilful generalship, and the most consummate science of the purely fighting portion of the machinery, are alike of no avail.

Yet how are the engineer and medical departments treated, as component parts of the same machine? In the former there are, at the present moment, eleven lieutenant-generals, six major-generals, fifteen colonels, and thirty-seven lieutenant-colonels, twelve of them with the brevet rank of colonel. In the latter there is one officer with the nominal rank of brigadier-general, and seven with that of lieutenant-colonel. By what principle of candour, fairness, or policy, can this be justified? It is not military authority that is contended for—that would be unreasonable to ask and impossible to grant—but it is the rank that adds dignity to office, and increases the usefulness of the officer in the performance of his duties.

One of the best officers the British army has ever possessed, and who bequeathed to it a legacy only surpassed by the Despatches of the Duke of Wellington, has said on this subject :

“The rank accorded to the medical officer does not injure, or even interfere with the military. Rank is of no intrinsic value in itself to a man of science; ~~but the~~ ^{rank} ~~connected~~ with the rank makes an impression on the soldier, which aids materially in giving force to medical authority, and, consequently, to medical utility. The soldier is accustomed to view things superficially, to estimate and judge by the exterior only; for, as he is not permitted to reason and resolve to principles, the science of the medical art is less regarded by him than the authority of the rank under which it is applied to him. For this reason, we venture to assert, that if the medical officer stands in what may be called a degraded rank in military estimation, the usefulness of the medical art will lose much of its value as applied to a military subject.

“The matter now under view is of some consequence to the interests of the army; and it is not, it is presumed, beneath the dignity of the higher powers of the State to consider it, if it be held to be a national concern to arrange the various departments of the army on a basis of justice and truth. Those who hold high official stations, and particularly those who wield the sword, are strongly disposed to depress men of science; and, among others, the medical department, which is a department of science, has been degraded of late years—at least, barred from rising to a rank suitable to its importance. But, be that as it may, the history of our most brilliant campaigns will not permit our most celebrated generals to say that nothing is due to the medical staff, when that staff is allowed to act according to its judgment.”

The work of Dr. Jackson, from which the above is extracted, heads the present notice, and had it been carefully studied by the War Ministers, general and staff officers engaged in the present contest, many of their most glaring and mischievous errors would have been avoided.

Similar sentiments have been expressed and recorded by Percy, Larrey, Deagenettes, and others of the most distinguished military surgeons of the Continent of Europe. The former presented a memorial on the subject to the Emperor Napoleon after the battle of Eylau, strongly advocating the assimilation of the medical to the engineer corps. It was not adopted because the plan was incomplete.

The disregard of the ordinary representations of medical officers in the

Crimea is notorious: it has produced its usual fruits. No body of educated men, whether commissioned or otherwise, are bound to submit to insult from authority, however high. Finding remonstrance useless, they ceased to remonstrate, and the unnecessary sufferings of the sick, beyond the control of those most interested in their welfare, has caused the indignation of the entire nation, and earned for us the contempt of Europe.

Had the smallest pains been taken during the long peace to profit by past experience and the counsels of such men as Dr. Jackson, in this age of education and progress, there is no reason why the British soldier of 1854 should have been as little able to take care of himself as his predecessor of 1804. The contrast drawn between the French and English systems in the early part of the century, reads as if it had come from the camp at Balaklava:

"In the French, utility and effect prevail over uniformity of appearance; knowledge of animal structure, and acquaintance with capacity of action in different structures, are deemed necessary to adjust and measure the effect. The exercises of manual and manœuvre are performed in the French army with a celerity and precision that cannot, perhaps, be exceeded; the explosions from the firelock astonish by close repetition. The effects of movements and evolutions in the face of an enemy, as studied in their reasons, are presented to the eye of the soldier, while under training, in such a manner that he may be supposed to comprehend the design and execute the measure—not passively as a part of a machine, but actively and with energy as an intelligent being. Besides practice in ~~manœuvres~~ and movement, which is the ostensible object in military training, pains are taken by the French tactician to lay the base of correct interior economy in the elements of the army; hence the recruit is instructed in the best manner of taking care of himself, with a view to enable him to maintain his efficiency as a part in an instrument of force. He is instructed, for instance, and scientifically instructed, in the best manner of dressing the raw material of the ration, so as to form a wholesome and savoury mess; and from this and other knowledge that belongs to interior economy, he suffers less privation and fewer hardships in the field than the troops of other nations similarly circumstanced—particularly than the British, who, the most brave, perhaps, of any soldiers in Europe, are the least competent of any to take care of themselves."*

The soldiery are no longer the scum of the country and the outpouring of the jails—the *enfants perdus* of society. They have advanced in intelligence to an extent that few anticipated before the publication of their letters, and the testimony of such men as the Rev. Mr. Osborne. There is no mystery in the art of cookery and the interior, to which Dr. Jackson refers, which could not be taught, and readily acquired by, the English soldier, who is as educable as any other class of his countrymen. But it is hopeless to expect any amendment in the lower ranks until the staff corps are properly trained; until the officers of the army as a body are real professional soldiers, instead of gay and gallant amateurs; until the entire system of military rewards and promotions is remodelled; until the ridiculous traffic in commissions is a thing of the past; and until professional and general competency, with distinguished service in the field, are the only passports to rank and command.

The third cause of failure has been one already more than once alluded to—the absence of inherent authority in the medical officers to act on

* Jackson, *op. cit.*, pp. 145, 6, ed. 1845.

their own responsibility, in any matter beyond the immediate and direct medical and surgical treatment of the sick and wounded. This is restricting their usefulness within the narrowest limit, and deprives them of the greatest element of success in all human actions—vigorous and direct action, based upon individual responsibility. Although it is not necessary to enlarge further upon it in this place; it cannot be too frequently repeated, or too earnestly dwelt upon.

The absence of a permanent sanitary commission in the British army, with no executive duties to perform, is to be regretted. It should be armed with power to consider all questions relating to the medical economy and *hygiène* of the soldier, in peace and in war; in Europe and in the tropics; in garrison, on the march, and in the field; in all matters relating to food, dress, equipments, exercise, parades, duties, and punishments, so far as they are liable to affect the health, and thereby impair the efficiency, of the soldier. It should examine, collate, and extract all that is valuable from the sanitary returns, which now answer no other purpose than to encumber medical officers with much harassing, detailed, and unnecessary correspondence—for it is a physical as well as a moral impossibility that they can, in existing circumstances, be made much use of by the Director-general and his personal staff.* It should, above all, when war is contemplated—and sufficient time is always allowed for such a purpose—collect and render available, in a condensed and practical form, all information, not least topographical, in its hygienic relations, procurable regarding the country or countries liable to be the seat of military operations. The purely military part of such information should be furnished by a properly organized staff corps; and thus, with all procurable information regarding the topography, military and medical resources, climate, and diseases of tracts of country about to be occupied, an army would take the field provided, as far as human means can make such provision, for accomplishing its political and strategic purposes with the least possible sacrifice of human life.

Such provision, or something equivalent to it, exists permanently in the armies of all the great military powers of Europe.

In the thirteenth volume of the 'Selection of Memoirs in Military Medicine,' published by order of the Minister of War in Paris, during the past year, is a report† by the Health Commission of the French army upon the countries which were regarded as the probable theatre of the future contest, before Silistria had driven back its assailants, and the lower

* Some such commission was proposed, many years since, by Sir James Macgregor, but rejected on the ground of expense, in consequence of which a vast mass of valuable records has accumulated, and been turned to no account.

† The paper is entitled 'Medical Instructions for the Army in the East,' is plain and practical, avoids all speculation and discussion, makes no attempt at deep research, and prepared the French surgeons for what they were likely to encounter.

"Every one knows," say the Commission, "that an army entering upon a campaign requires special precautions to preserve it from the destructive influences upon health which may result from a sudden change of habits, mode of living, fatigue, and irregularities, which this novel state either necessitates, or inevitably has a tendency to produce. Memorable examples have demonstrated the certain efficacy of these precautions every time they have been observed, as well as the disasters which their neglect or oblivion involve."

This now sounds prophetic, for, with the exception of Dr. Smith, no person in authority in the English army, or in its home administration, would appear, from the result, to have

valley of the Danube, with the adjacent countries, were likely to be occupied by the French army. It embraced the medical topography and meteorology of the chain of the Balkan, Bulgaria, Roumelia, Wallachia, and the valley of the Lower Danube. It also considered the diseases, endemic and, epidemic, of those countries; their pathology, *hygiène*, and therapeutics, as regarded sick and wounded soldiers; and concluded with

bestowed a thought upon the subject. The report concludes with a "Summary of the precautions to be taken for the preservation of the health of the Troops in the East."

"The Council of Health has been charged by the Minister of War to examine the precautions most proper for maintaining the health of the troops in the country to which they are called to make war. This Council has drawn up detailed instructions, from which the following particulars are extracted as necessary to be observed with the greatest care:

"1. It is necessary to be always so clothed as to be proof against the sudden chills to which one is liable, at all seasons, from the abrupt changes of temperature which very frequently happen in nearly all parts of this country.

"2. In summer, the best protection against sun-strokes, which are often very dangerous, is never to leave shelter without having the head covered.

"3. Cleanliness of persons, clothes, and dwellings, is imperiously required by the nature of the climate.

"4. Whenever practicable, the face, and particularly the eyes, should be frequently washed daily, after exposure to dust.

"5. The feet should *not* be washed with cold water, especially when heated after a march.

"6. The greatest care is needed for protection against the freshness of the nights, even when the heat is extreme; it is dangerous to remain clad only with the shirt during the night.

"At the bivouac and in the tent, the soldiers should be very carefully covered.

"7. When the camp is pitched near a marsh, a tank, pools of stagnant water, or a valley, the ~~the~~ openings of the barracks or tents should be in the opposite direction. In these bivouacs every possible means should be employed to counteract the ~~injury~~ ~~injurious~~ vapours exhaled by such foci of infection. At night, the openings of the barracks, with the exception of those indispensable for ventilation, should be closed.

"8. It is wrong to sleep in immediate contact with the ground; perfectly dry substances, not easily permeated by moisture, should be interposed. For this purpose fresh branches or vegetable matters should never be used.

"9. Water, drunk in large quantity, is always injurious. If, after a fatiguing march, a stream of water is met with, thirst must be sparingly satisfied, and the water reserved for subsequent use.

"10. When only a small quantity of water is procurable, instead of swallowing it, the mouth should be gargled as long as possible, and the water rejected as soon as it is warm.

"11. When reduced to the necessity of drinking stagnant water, it should, by way of precaution, be strained through a cloth, to separate leeches imperceptible from their smallness, and which it is very dangerous to swallow.

"12. A mixture of wine and water, brandy and water, or infusion of coffee and water, is always an excellent drink, taken in moderation. It should be mixed at the time of use, and not prepared beforehand, as it in that case becomes heated, changes, and no longer fulfils its purpose.

"13. When salted meat and fish are substituted for fresh meat, they should be soaked before cooking, and when practicable, mixed with a certain amount of vegetables.

"14. Condiments, in small quantity, are good seasoning; in excess, they irritate the stomach, and render thirst more difficult to bear.

"15. Saffron increases the digestibility of rice and flour; it is particularly useful with maize.

"16. Food should always be taken before a march.

"17. Before and after guard-mounting it is very useful to drink a moderate quantity of warm water, with a little brandy or infusion of coffee in it. Night guards should always be well clothed.

"18. Sickness should at once be reported to the medical officer.

"19. These recommendations shall be published in general orders. Every commanding officer will take care that they are read to the troops once a week at least, and that the officers superintend and direct their execution."

Many of these directions read like truisms, and yet experience shows that they require to be enforced, for everything must be made plain to the soldier.

The above extract is given without curtailment, as showing the manner in which the functions of a permanent sanitary commission may become of importance in war.

So entirely convinced are we upon the matter, that we trust ere long to see such commissions in existence in the English and Indian armies, and composed of some of the best officers in their respective services.

a few valuable general recommendations to the medical officers of the force.

In this matter, however, the French were anticipated by the English, and the wisdom and forethought of the director-general were as marked in this as in all the other arrangements which he was permitted to carry out.

As soon as it was known that a field force was to be sent to Turkey, upon the advice and recommendation of Dr. Smith, deputy inspectors-general Dumbeck and Linton, and staff-surgeon Mitchell, were despatched to examine and report upon the countries north and south of the Balkan chain, the tracts between Adrianople and Gallipoli, and, in fact, the whole line that would have been the seat of war; had the Russian generals succeeded in their original plan. Each of the experienced officers above-mentioned furnished detailed reports regarding the diseases of the countries referred to, for the use of the chief medical officer and of the general commanding the army.

Portions of these reports have, from time to time, appeared in one of the medical journals published in London.

In addition to this, Dr. Smith caused to be written and published valuable notes on diseases in Turkey by Dr. Schulkof and Dr. Bryce, with useful practical memoranda by Dr. Bryson and Mr. Drummond. No medical officer can have studied them without advantage.

Having thus drawn attention to what we believe to be the charges made against the medical department, and the causes to which the deficiencies undoubtedly existing, appear to us to be due, we next proceed to the means by which we think they may be avoided for the future. These have, in some measure, been already incidentally referred to; but as they are sufficiently important to be developed more in detail, we shall make no apology for dwelling more particularly upon them.

They embrace the future supply of men and material, the selection and promotion of officers in the department, the places they should occupy in the military body, and such other incidental considerations as may be involved in the discussion of the points above-mentioned.

The scientific branches of the army, under which may be classed the Engineers, Artillery, and Medical Departments, are, with the exception of the very faint efforts made at Sandhurst to train officers for staff employments, the only portions of the military machine in which anything approaching to systematic organization has been attempted. Unlike the raw material for British heroes, they cannot be created upon emergencies; hence the greatest care is requisite that they should, in time of peace, not only be perfect in themselves, but be so established as to be capable of indefinite expansion, to meet the demands of war.

The radical defects of the medical organization at the opening of the campaign have been already mentioned. We shall recapitulate them. They were—THE ABSENCE OF PROPER HOSPITAL ESTABLISHMENTS, INCLUDING AN AMBULANCE CORPS; THE NON-EXISTENCE OF THE INDISPENSABLE CLASS OF APOTHECARIES;* THE ABOLITION OF THE STAFF OF

* To prevent misapprehension, we may observe that the duties of the apothecaries are not to treat the sick, but to supply the medicines, instruments, and other appliances used by the surgeons and assistant-surgeons.

PURVEYORS, ESSENTIAL FOR THE PROVISIONING OF THE SICK; THE COMPLETE SUBORDINATION OF THE MEDICAL TO THE MILITARY AUTHORITIES, EVEN IN MATTERS OF PROFESSIONAL DETAIL, WHICH THEY ALONE COULD PROPERLY UNDERSTAND AND CARRY OUT.

The first of these is understood to be in the course of organization upon an ample and liberal scale, that will include all the essential ministerial agency, without which no military hospital or establishment, in or out of the field, can possibly be in a state of efficiency.

Under the old system, the selection of the unfit instruments allowed by the Government did not rest with the surgeon; in most matters they were beyond his control; and if capable of aiding him, he was forbidden by the regulations to employ them in any professional duty.

The general reader will best understand the destitution of the army in this important particular, when told that it was exactly parallel to that of an engineer corps without a trained band of sappers, or a regiment of artillery without gunners. It is manifest that, in such circumstances, all the skill and science of the former would be unavailing in the construction of redoubts, field-works, parallels, trenches, and the other machinery of siege operations; and that, if the latter had to limber and unlimber their guns, to load, fire, stop vents, cut fuzes, and discharge the other practical details of gunnery, the siege of Sebastopol could have advanced little, and that of Troy would be rivalled in length, if not imitated in results.

Yet an analogous feat—the capture of a fortress without sappers or gunners—was expected to be performed by the medical department!

The most important of the changes will be, it is hoped, to place this hospital corps beyond the reach of colonels and adjutants, and to reserve it exclusively for its own proper and most important duties. From it should be selected the members of the ambulance corps—strong, steady, trained soldiers, accustomed to accidents and injuries in the wards of an hospital, habituated to handle fractured and contused limbs without inflicting unnecessary suffering, and even able, upon emergencies, to apply tourniquets, and arrest hæmorrhage temporarily, until the scientific skill of the surgeon can be brought in contact with the wounded man.

The ambulances should be of the lightest materials consistent with safety; be so constructed as to be capable of transport over rough, difficult, and roadless countries; be adapted to receive the wounded in the recumbent or sitting posture with the least possible difficulty, so as to place them *cito, tuto, and may-hap jucunde*, within reach of professional aid.

It should be able closely to follow the movements of an advancing or retreating force, and rescue the wounded before hæmorrhage can, as it so frequently does, convert trifling into serious injuries.

Such a corps and machinery—with the modern mechanical improvements that experience in such matters has suggested—as Larrey organized during the campaign on the Rhine, and which was hailed with such unmingled satisfaction by the soldiers of the French army, is what is required.

The nature of the country would, in some cases, modify the means of transport. The mule panniers of the French army, or the camel panniers of India, would, in certain circumstances, be preferable to all

wheeled conveyances. It should be the duty of the medical staff to ascertain, and they should be empowered to provide for, all such contingencies, before the army takes the field in a new country. The packing up of the instruments, and employment of bandsmen in the carrying of the wounded, has always appeared to us to be a foolish and objectionable measure, as well as in itself ill-calculated to accomplish the end in view.

The influence of martial music is very powerful upon the soldier, and is not sufficiently employed in the British army as a moral means of management. The band, instead of being a lordly luxury for the officers, should be liberally maintained by the State for the use of the soldier. This, as well as every other point, however apparently unimportant in the organization of an army, is well understood in all Continental armies.

To return, however, to our more immediate subject—the next deficiency was the absence of a staff of apothecaries. This has also now been supplied, and when its subordinate dispensers are collected and trained, will doubtless prove of the utmost use to the present overwrought and under-handed medical establishment. In the selection of its members, their training and qualifications, they should be assimilated to the *pharmaciens* of the French and Belgian armies, but should be under the direct control of the medical authorities.

From the apothecaries to the supply of medicines, and the other means and appliances of military hospitals, the transition is easy and natural. There should, we incline to think, be a great central store for the accumulation and rapid and regular distribution of such essential munitions of war, similar to the medical stores at the three Presidencies of India. The circumstances of the case are not perhaps exactly parallel, because England is herself the great centre of supply and manufacture of all such material. Yet recent experience has shown, in the case of minié rifles, and a thousand essentials for warfare, that it is unwise and imprudent to rely upon the general market; and that, to be fully prepared for emergencies, the Government should always have at command the means and appliances necessary. The result would be one of economy, as well as efficiency; and instruments and medicines would be procured both better and cheaper, under the system of the East India Company, than as a department of the Medical Board of the Queen's army. They could always be kept ready packed and sorted for detachments of all strengths, so that a regiment, a division, or an army could be supplied with a celerity and precision unattainable under any other system. The subject will be again referred to in a subsequent portion of this sketch, when the medical storekeepers of the Indian army, and their duties, are considered.

If it be essential that the medical appliances of hospitals should be always at hand, and under the directly responsible control of the medical staff, it is not less so that the commissariat arrangements for the sick and wounded should be equally *special*, and proper to the department. The old purveyor's regulations appear to be sufficiently plain and practical, and are probably again adopted; but it would be better that the hospital department should be altogether independent, and not compelled to resort to the general commissariat of the army for the supply of "fresh meat, bread, wine, and every other article for the consumption of the hospitals."

It must simplify accounts, prevent irregularity of supply, and might easily be managed, by the introduction of a little of the tact and common sense which characterize the arrangements of French hospitals in such matters.

When the arrangements for the issue and supply of medicines and hospital materials, the training of an efficient staff of hospital attendants, including an ambulance corps, and the organization of a hospital commissariat, are complete, the whole machine will work harmoniously, if it is placed under the *absolute control* of the medical authorities.

But, if military interference and supervision in the details of arrangements are introduced, and the real control and authority are thus removed from the hands of the medical staff, the old irregularities will recur, and all attempts to render the medical department an honourable, useful, and efficient branch of the army will assuredly fail, as they have done heretofore. Like causes, in similar circumstances, produce like effects; and the experience of the past will have proved little useful as a guide for the future, if the whole system is not radically changed. It has been tried, and the first rude contact with war has broken the glittering bauble. The medical corps has never really had the management of its own concerns. It is only fair that it should at least have a trial of its own.

Before quitting the subject of the Queen's Medical Service, we have a few words to say on the modes in which the members of that service are selected, promoted and educated for their special duties.

The existing system of appointments to, and promotions in, the Royal medical department, is not susceptible of amendment, for they are not suited to the spirit of the times in which we live, and must be altogether changed.

There can be no doubt, without questioning the fairness and integrity of the head of any department at present entrusted with patronage, that all branches of the public service, in every one of its numerous ramifications, should be thrown open to the whole body of the educated classes in the United Kingdom. Recent events, the memory of which will long outlive the deep feeling of national humiliation now experienced at the failure of the public administration, in departments where it was perilous to permit the existence of incapacity, have shown, that the corruption and nepotism of the past must be swept away with a firm, unwavering hand. The right to serve their country belongs to all her sons alike, and the country herself is entitled to the services of those best qualified to administer with credit and efficiency the duties entrusted to them. It is wrong in principle that any favour should have to be asked on such occasions. It is a sacred and a public right, which public opinion should, by all constitutional means, enforce, from the highest to the lowest offices of the State.

Purely personal and household appointments should, as at present, for obvious reasons, be matters of personal selection. Gold, silver, and all other varieties of sticks, may retain the *status quo ante*, without much fear of damage to the national reputation. But there the line should be drawn; and from the First Minister of the Crown to the lowest public servant in the most distant colonial possession of the country, the fittest men should be selected for the various offices, if there be any

means of gauging intellectual and general capacity, and of making such selection.

This is a great political problem, which appears likely to be partially solved at present, and which it is neither our province nor our wish to investigate.

With respect to the medical branch of the army, the solution has already been furnished, by what has been done in the case of the Indian civil and medical services. The magnitude and importance of the services performed by the officers of those branches of Indian administration, range from the government of vast provinces to the minutest ministerial details connected with their different offices, and their duties are inferior in gravity and responsibility to those of no other public servants in the civilized world.

In France, again, the medical staff is, like all other scientific branches of the army, recruited by *concours* from the entire body of professional men in the country who are willing to enter the military service. There is no lack of candidates and competence. The officers have an interest in the well-being of their department, and the State is well served in consequence.

The question of promotion is a more difficult one, but even here the difficulties are by no means insuperable. In France three elements enter into the question—*concours*, seniority, and selection; but their system is faulty, and is deservedly censured by their own best officers, inasmuch as the real selection rests with a mixed tribunal, in which the professional element is secondary. There can be no doubt that professional merit and competency can only be really ascertained by professional men, and that the introduction of an exoteric element, of high authority, with no inherent means or capacity of forming a right judgment, is fatal to the integrity of any scheme of promotion so framed. The *concours* applies only to the lower offices in the hierarchy, and to the professorships in the military medical schools.

The worst of all claims, when unaccompanied by capacity and competence, is that of seniority; yet, when combined with those essential elements of advancement, it would be unwise, illiberal and unjust, to disregard it. It should, in all cases, be secondary to merit where administrative duties, requiring more than the average of professional attainments and mental and bodily vigour, are in question. In mere executive duties, such as fall to the lot of the regimental surgeon, the point is probably not quite so essential, yet even here it is lamentable to think of the amount of preventable misery and mortality, entailed by the appointment of an ignorant and incompetent surgeon.

The bane and curse of the Indian medical department has been the seniority system, which still flourishes there in all its vitality and vigour, as will be shown more in detail hereafter.

The least objectionable system at present in operation, with which we are acquainted, is that of the Belgian army, as promulgated in the 'Loi sur l'admission et l'avancement dans le Service Sanitaire de l'Armée et de la Marine,' in March, 1847. It is as follows:

"ART. 4. L'avancement aux grades de médecin de bataillon de deuxième et de première classe, et de médecin de régiment, aura lieu, moitié au choix, moitié à

l'ancienneté, à moins l'insuffisance de sujets capables parmi les plus anciens du grade immédiatement inférieur.

"L'aptitude des candidats sera constatée, pour les médecins-adjoints, par des rapports périodiques des chefs de service, et par les inspections générales; pour les médecins de bataillon par un examen.

"Ces rapports et ces examens ne porteront que sur des connaissances pratiques. Ils auront, en outre, pour objet, en ce qui concerne les médecins-adjoints, l'hygiène militaire et l'administration pharmaceutique."

The choice of the superior officers rests with the King, and in so small an army as that of Belgium, where the personal character and qualifications of every officer of any rank and standing must be well known, unfit instruments are not likely to be selected by the Sovereign, particularly when he possesses the most ample means of knowing all the men of real mark and merit, from the examinations passed in rising from the inferior ranks.

Some system of promotion by examination, to regimental surgeoncies, was apparently tried, and failed, in the British army a few years since. We are too imperfectly acquainted with the matter to be able to assign a reason for the failure; but, if it were properly conducted, upon so sound and rational a basis as that of the Belgian system noted at foot,* there is no more reason why it should not have succeeded, than that the French army before Sebastopol should be better fed, clothed, and organized, than the British force in the same position. The resolution to overcome the difficulty would soon cause it to disappear.

We cannot help thinking that the perfection of the regimental system of the English army has been considerably over-estimated, and that it does not sufficiently prepare the officers and men for acting in large bodies with the precision and unity necessary for complete success. It has, doubtless, many and great advantages in the *esprit de corps* which it fosters, and the close personal acquaintance and companionship that it maintains between the men and their officers; but it has, like our imperfect military system generally, a tendency to narrow and contract the views of all concerned. It is like all working in circles. The attention to minute micro-

* MESURES D'EXECUTION DE LA LOI D'ORGANISATION DU SERVICE DE SANTE
RELATIVES AUX EXAMENS.

(Arrêté Royal du 20 Mai, 1847.)

LEOPOLD, &c.

Vu la loi du 10 Mars, 1847, relative au rang et au mode d'admission et d'avancement des officiers du service de santé de l'armée et de la marine;

Considérant qu'il est nécessaire d'arrêter les mesures d'exécution qui se rattachent aux examens auxquels les officiers de santé de divers grades et catégories sont soumis;

ART. 1. Nul ne sera admis à subir l'examen d'un grade supérieur, qu'après avoir servi pendant deux ans dans le grade immédiatement inférieur.

ART. 2. Les examens auront lieu à Bruxelles, une fois par an, à l'époque qui sera fixée par notre ministre de la guerre. Ils seront annoncés trois mois d'avance.

ART. 3. Chaque des commissions d'examen sera composée de trois ou de cinq membres, désignés par nous.

ART. 4. Les questions à résoudre, les sujets à traiter et les opérations à pratiquer ou à exécuter seront désignés par la voie du sort, conformément au programme à arrêter par notre ministre de la guerre.

ART. 5. La commission constatera l'aptitude ou l'inhabileté de chacun des concurrents à occuper le grade pour lequel l'examen a lieu. Elle réglera le rang de ceux qui auront été jugés aptes. Ses décisions seront prises à la majorité des voix, au scrutin secret, après délibération et discussion.

ART. 6. Les procès-verbaux des opérations des commissions d'examen seront inscrits dans

scopical details is unfavourable to the more enlarged views and the general laws which regulate the actions of mankind in the mass:

"If viewed according to the reason of things, the genuine military principle, and many of the current practices of the day, are in direct contradiction to each other."*

With the purely military bearing of the question we have nothing to do, but it is of consequence in its relations to the medical affairs of the army. The assistant-surgeon on first joining the army, with the exception of the short period he may pass at Fort Pitt, has no opportunity of learning his duties in such manner as to fit him to encounter the emergencies, that will rise up in the path of his future career.

Among the indisputable advantages possessed by the French army is the existence of two classes of military medical schools of instruction.

un registre à ouvrir expressément pour cet objet et qui restera déposé dans les archives du département de la guerre.

ART 7. Après chaque examen, il sera délivré par la commission à ceux qui y auront satisfait un bulletin qui en indiquera la date et le résultat, soit qu'ils l'aient passé d'une manière satisfaisante, avec distinction, ou avec grande distinction.

PROGRAMME DES EXAMENS.

LE MINISTRE DE LA GUERRE,—

Vu l'art. 4 de l'arrêté royal du 20 Mai, 1847, ainsi conçu: "Les questions à résoudre, les sujets à traiter et les opérations à pratiquer ou à exécuter, seront désignées par la voie du sort, conformément au programme à arrêter par notre ministre de la guerre."

ARRETE:

ART. 1. Les programmes des examens prescrits par la loi du 10 Mars, 1847, pour les officiers de santé de divers grades et catégories, sont fixés comme suit:

A. Pour le Grade de Médecin de Régiment.

Traitement de six malades pris parmi les fiévreux, blessés, ophthalmiques et vénériens:—redaction des histoires des maladies et des détails nécropsiques, s'il y a lieu;—discussion de vive voix ou par écrit, sur les questions que pourra soulever l'examen des malades;

Pratique de trois opérations chirurgicales sur le vivant ou sur le cadavre, après avoir exposé les motifs qui peuvent les nécessiter, décrit les divers méthodes et procédés, et motivé la préférence donnée au mode opératoire adopté.

Application de deux appareils au moins, et appréciation de vive voix du mérite respectif du mode de désignation préféré.

Visite de deux hommes au moins pour s'assurer s'ils sont propres au service, et, en cas d'affirmative, à quelles armes ils conviennent spécialement.

Visite de deux militaires au moins, atteints d'infirmités ou de maladies chroniques, à l'effet de constater les causes patentes ou possibles de leurs affections, leur symptômes distinctifs, les chances probables de guérison complète ou incomplète; redaction, s'il y a lieu, des certificats détaillés exigés pour leur libération du service.

Solution par écrit, de deux questions d'hygiène militaire, se rapportant spécialement à l'emplacement, l'établissement et l'aménagement des casernes, des hôpitaux, des prisons, des campements.

Appréciation des qualités des denrées alimentaires à l'usage du soldat, aidée au besoin de l'analyse chimique.

B. Pour le Grade de Pharmacien de Deuxième Classe.

Deux préparations, l'une chimique, l'autre pharmaceutique:—exposé, de vive voix ou par écrit, des phénomènes dont elles sont accompagnées.

Deux analyses qualitatives.

Solution, par écrit ou de vive voix, de trois questions au moins sur les instructions relatives au service et à la compatibilité pharmaceutiques.

C. Pour le Grade de Pharmacien de Première Classe.

Deux préparations chimiques et l'exposé, par écrit ou de vive voix, des phénomènes qui les accompagnent.

Deux analyses quantitatives.

* Jackson, op. cit., p. 356.

The elementary schools of the second degree are called military hospitals of instruction, and there are three of them, placed respectively at Strasbourg, Metz, and Lille. The school of the first degree has received the name of the hospital of perfectioning, or finishing, and is the celebrated military hospital of the Val de Grace, in Paris.

The professors in these schools are all military surgeons, selected by *concours*. The details of their structure and management are well described in M. Bégin's valuable essay 'On the Military Medical Service in France.' In that military country, now our fast and firm ally, bound by ties of common danger, and baptized in the blood of the battle-field, it is recognised as an axiom, that "the degree of perfection of military medical science, is the true measure of the importance attached to the preservation of the soldier."

The Val de Grace, with its splendid museum, its valuable library, its commodious laboratories, theatres, and dissecting rooms, and its perfect commissariat and pharmaceutical arrangements, contains within itself the means of completing the professional education of the young military surgeons, in such manner as cannot fail to be of the utmost service to them in their after career.

A similar institution could easily be organized in England, and through it all candidates who have been selected for appointments in the army should pass, so as in no case to be drafted to the independent duties of their profession, until the strictest examinations, held periodically, had tested their fitness. Such an institution would be to military surgeons what camps of exercise are to their brethren in arms. Civil hospitals and ordinary courses of medical instruction cannot supply its place, for reasons too obvious to be discussed in these remarks.

The existing regimental hospitals, at home, at least, afford no such opportunities for all branches—professional, pharmaceutical, and commissariat—to become practically acquainted with their functions, so as to fit them for field service.

It only remains for us now to say a few words upon the organization of the department as at present constituted, which, although superior to the order of things which it succeeded, is not what the medical profession, as a scientific branch of the army, has a right to expect.

The promotion is too slow to secure men of vigour and ability for the higher offices of the department, and the proportion of field officers is so glaringly disproportionate to the strength and wants of the service, as to place it in painful and injurious contrast with the other scientific branches.

The expensive professional training of a medical officer is as an outlay of capital, more than equivalent to the purchase of a company; and the later age at which he, in consequence, enters the service, entitles him to much earlier promotion than he now obtains to place him on a par with his brother-officers. Sir George Ballingall has recently pointed out the necessity of providing such encouragement for the retirement of those who are no longer fit for active duty, as may secure for service active men, and prevent the accumulation on the muster-rolls of old men who are unfit for employment in the operations of war. It has been remarked by more than one writer from the camp at Sebastopol, that there is not a grey head to be seen in the French camp!

In the Belgian army, the preliminary studies of a medical graduate admitted to the army are reckoned as equivalent to six years of service. An assistant-surgeon can be promoted to a battalion surgeoncy of the second class after two years' service. A battalion surgeon of the second class, after two years' service in that rank, can be promoted to a battalion surgeoncy of the first class. Again, in two years, the battalion surgeon of the first class may become a regimental surgeon, and he in turn may become a garrison surgeon in four years. The garrison surgeon, after three years' service as such, may obtain the grade of "*médecin principal*." The "*médecin principal*" may become "*médecin-en-chef*" in two years, and the latter inspector-general in two years more. It is true that the numbers given are the minimum allowed, yet, it is equally certain, that an officer of energy and ability may rise to the head of his department in seventeen years, or before he is forty years of age.

The military rank of the Belgian medical officers is the following:

Inspector-general ranks with a	Major-general in the army.
The <i>médecin-en-chef</i> (chief physician)	Colonel.
The <i>médecins principaux</i> (principal physicians)	Lieut.-colonels.
Garrison surgeons	Majors.
Regimental surgeons	Captains of the first class.
Battalion surgeons of the first class	Captains of the second class.
Battalion surgeons of the second class	Lieutenants.
Assistant-surgeons	{ Sub-lieutenants.
	{ Ensigns.

In this scale there is something like an attempt to assimilate the medical to the military service. The head of the medical department is the inspector-general, and he is, in all respects, treated as a major-general.

In the French army, a student in medicine is eligible for the commission of assistant surgeon of the second class on producing proof of being a born or naturalized Frenchman; of having fulfilled the law of recruiting; of possessing the title of doctor of medicine; of producing from the dean of the faculty in which he has graduated, a certificate of the manner in which he has obtained his degree, and of having obtained the remark *satisfactory*: of morality and good conduct during his pupillage; of not being more than twenty-six years of age; of passing satisfactorily a special examination; and of labouring under no physical infirmity to unfit him for service.

Having fulfilled all the above conditions, the assistant-surgeon of the second class must serve for two years before he can become an assistant-surgeon of the first class. The first class assistant-surgeon must serve as such for two years before he can become a surgeon (major) of the second class; and the latter requires at least four years' service in that rank before he can be promoted to a surgeoncy of the first class. The first class surgeon must serve for three years to become a principal (principal medical officer) of the second class, and the principal of the second class for at least two additional years ere he can attain the rank of principal of the first class. A principal of the first class is eligible for an inspectorship after three years' service in his existing rank. The period of promotion by length of service may be diminished by *one-half in time of war, or by service in the colonies*. The only other means of dispensing with the regulations above

related is—1st, For an act of devotion and courage, duly certified and published in the army or division order of the day; and 2ndly, When it is not possible otherwise to fill up vacancies. Thus, in time of peace, a French medical officer of ability, experience, and acquirements, may mount to the highest round of the ladder of preferment in sixteen years; and in time of war, in half that space. Distinguished bravery and devotion may procure his immediate advancement.

Is it a wonder that, under such encouragement, French hospitals and ambulances are models of imitation, and that the French army is able to transport 2000 British sick in comfortable panniers, while our ambulances are "nowhere"?

The comparative scale of ranks in the French army is as follows—viz.:

1. Inspector-general	General of brigade.
2. Principal inspector	Colonel.
3. Principal	Lieutenant-colonel.
4. Major (surgeon)	Chef de bataillon.
5. Senior assistant-surgeons (aides-majors.)	(Major in the English army.)
6. Sub-assistant surgeons	Captains.
7. Pupillary sub-assistants (élèves sous-aides)	Lieutenants.
	Sub-lieutenants.

Here, again, although some of the classes have no existence in the English army, the advantage is in favour of our neighbours, and there is something like a military scale in the disposition.

In the English army, the following is the scale:

Director-general	Brigadier.
Inspector-general	Colonel.
Deputy inspector-general	Lieutenant-colonel.
Staff surgeons of the first class	Majors.
Regimental surgeons and staff surgeons, second class	Captains.
Assistant-surgeons	Lieutenants.

The greatest objections to this scale are, that the lower ranks are altogether out of proportion to the higher; that the rank itself is a sham, and not a reality; that the passage from one class to another is too slow and indeterminate, and that promotion is based upon no principle which secures a deserving officer from neglect or supercession; and that it is not calculated to place men in the prime of life and energy in the highest offices of the hierarchy.

In all matters relating to military honours, funerals, quarters, prize money, passage in transports, and uniform, there should be no class distinctions drawn, as there too frequently are, in favour of the general and against the medical staff. Their own pensions and those of their widows should be assimilated to those of their brother officers of corresponding rank and standing.

In a word, the medical should be treated as a substantive branch of the army, and placed, as far as circumstances will permit, upon the same footing as the engineer corps.

To sum up, in a few sentences, the result of the arguments imperfectly developed in the foregoing pages:

1. Without in any degree diminishing its subordination in the military corps in all field and strategic operations, and in regiments, the medical department should possess the power of managing its own affairs, independent of external interference.

2. It should possess within itself, and liable to no deviation from its own peculiar functions, its staff of apothecaries, purveyors, dispensers, and assistants of all classes, down to the lowest functionary necessary in a hospital.

3. Its ambulances, stores, and appliances of all descriptions, should be under its own control and management.

4. Its rank in the military hierarchy should be distinct, defined, and substantive; it should possess its proportionate share of field and superior officers; it should participate in military honours in the same manner as all other branches; its dress should not remain ridiculous and unsuitable; and in all regulations regarding pensions, and allowances of every description, it should be fully on a par with all other branches of the army.

5. Its promotion should be so regulated as to secure rapid preferment for the energetic and deserving, and a comfortable retreat, after a fixed period of service, for those who wish to retire or are unfit for active duty in the field.

Having thus, however imperfectly, attempted to place in a clear point of view the defects and just requirements of the medical department of her Majesty's army, we will now proceed to that of the East India Company.

The medical department of the three Presidencies of India are similarly constituted, upon an antiquated principle, quite unsuited to the wants and to the development of the real usefulness of the profession. They consist of senior surgeons, surgeons, and assistant-surgeons, with a subordinate staff for the minor duties of civil and military hospitals. They are all essentially seniority services, and although attempts have been made at various times to introduce a principle of selection for the higher staff appointments, they have invariably failed to attain the desired end. It is lamentable to know that this has resulted more from narrow professional jealousy, than from any indisposition on the part of the Government to fulfil faithfully the good intentions of the authorities in Europe. But more of this anon.

The senior surgeons are all officers of thirty years' standing and upwards, and rank as majors in the army. The three first on the list in general constitute the medical board, and are designated respectively physician-general, surgeon-general, and inspector-general — titles of no meaning with reference to the duties they have to perform. They are classed comparatively as brigadier-generals, a grade which has no real existence, as a substantive rank, in the army. Their duty is to receive all returns and reports, to regulate the medical affairs of the Presidency, and to act as the professional advisers of the Government in all things pertaining to the medical department generally.

It is no libel upon the conscript fathers to state what is patent to and acknowledged by all in India who know anything of the subject—viz., that for all purposes of usefulness they are absolutely and essentially impotent. They attain their position by sheer longevity, without the

smallest reference to capacity or fitness; and, in many instances, have been notoriously inefficient from the very commencement of their career. They become the senate of the medical corporation when, in the course of nature, the mental and bodily faculties are fast declining, and the judgment becomes as obtuse as the senses. Even if, in their brightest days, they were the most active, intelligent, capable, gifted, and learned members of their class, they would be unsuited for active administrative duties at the period of life when they usually mount upon the medical throne.

The truth is, that the instances in which the really distinguished members of the profession have either lived, or remained in the service long enough to become members of the board, are rare, although that position has been, and is, *occasionally* occupied by some of the most efficient officers in the department. They are, however, *rari nantes in gurgite vasto*.

The men of mark, and of high intellectual development, are almost invariably the earliest to suffer the inevitable penalty of decay and decline. It is so in all professions, and the medical calling is no exception to the rule. The easy, quiet, good-natured man of moderate capacity, has, in all circumstances, the best chance of outliving his mercurial, hard-working contemporary. The result is, that the nominal heads of the service, however amiable and excellent as individuals—and many of them have been, and are, most worthy, estimable men—are not so respected in their public capacity as to command the confidence of their rulers, or of those serving under them. The patronage of the department is not in their hands; they are seldom, if ever, consulted upon any question of importance; their advice and recommendations are set aside, without scruple or delicacy; they have no means of controlling or encouraging professional exertions; and are, in every sense, a detriment to their branch of the army.

If the constitution of the board be, as few will now-a-days dispute, radically defective, and mischievous in point of policy, it is still less defensible as a means of rewarding long and faithful services. The old and tried servant is deserving of every consideration from the State, to which the best years of his life have been honestly and conscientiously devoted. It is in the ease and dignity of retirement that the recompence of a well-spent life of usefulness should consist, and not in the performance of duties, for the efficient discharge of which the greatest mental and bodily vigour is needed. To place an old man in such a position is as cruel to him, as it is injurious to the public interest.

The executive officer of each of the boards is a secretary, by whom the current duties of the office are performed, and from whom any efficiency possessed or exhibited by the board, really emanates. The character of this functionary generally determines that of the body, whose right-hand and mouthpiece he is. The secretaries have generally been able and efficient officers, but for whose exertions the venerable and effete institutions would long since have been consigned to the oblivion which has gradually overtaken them in Europe. They have little real power of doing good, experience considerable difficulty in reconciling the crotchety differences of their chiefs, and are too much absorbed in current details of office, to be able to produce any work that would tend to elevate or advance their profession.

The general inspection of hospitals in the different circles of command into which each Presidency is divided, is performed by the senior surgeons next below the members of the medical board on the list. They are designated superintending surgeons, and rank as lieutenant-colonels. They are invariably appointed in the order of seniority, and seldom attain their rank before they have served for thirty years. In Bengal, the Punjaub, and Burmah, there are 11 superintending surgeons; for Madras, 10; and for Bombay, including Scinde, 5. The correspondence regarding all professional matters in their respective divisions passes through them, and they are supposed to visit and inspect the various hospitals in their circles, as often as practicable. The professional supervision so exercised is usually an utter farce, for it rarely happens that the senior officer is even equal in attainments to most of those serving under him. Being selected by seniority, without the smallest reference to qualification, their interference, as a general rule, would, in the majority of instances, be more mischievous than useful. Even physical inability to discharge their duties has not been a barrier to their promotion, and the blind, deaf, and infirm have been known to have held an office, which, in any other army in the world, would have been bestowed only on the most active, deserving, and efficient members of the corps. The intention of the home Government has always been to make this important staff office essentially one of selection, seniority only constituting a preferential claim, and being held subordinate to capacity and qualification. This just and wise measure has, heretofore, proved a dead letter, chiefly from causes little creditable to the profession itself. The supreme Government, wisely acting upon the maxim that, in purely professional matters, professional men alone are competent judges, has, on one or two recent occasions, been reputed to have consulted one of the medical boards as to the propriety of promoting officers, generally believed to be useless and inefficient, to higher offices in the department. The conscript fathers so consulted are believed to have been destitute of the courage necessary to give an honest opinion upon the subject, and the result was the elevation of men notoriously unequal, from age and infirmity, to discharge the most ordinary duties of their offices.

The clamour raised in the very few cases in which the Government has departed from the rule of seniority, appears to have deterred the ruling authorities from giving full effect to a principle of selection that was intended to reward merit and efficiency, with due regard to the claims of mere length of service. With a single capable, responsible head of the medical service, possessing the confidence of the Government, and proof against the idle clamour of the drones and incapables, such discreditably derelictions of duty could not have occurred.

Between the superintending and assistant surgeons there is no intermediate substantive rank, similar to that of staff surgeon of the first class in the Royal army. All are surgeons, ranking as captains, until they have served for thirty years, and then classed as majors, without any additional pay, emolument, or consideration of any kind. The average interval thus passed is generally not less than fifteen of the best years of a surgeon's life in India, and it is during this period that the majority of the most efficient retire. Some inducement is necessary to tempt men

in the prime of life and usefulness to remain in the service. Few are inclined to bide their time for promotion, until they are unfit for active exertion; hence the greater number leave at the very time their experience renders their services most valuable. This very detrimental exodus might easily be prevented, without much cost, and with great benefit to the State.

The assistant-surgeon, shortly after his arrival in Bengal, after being supposed to have picked up some scraps of tropical pathology and therapeutics at the General Hospital, is either appointed to do duty with a European regiment, attached to a native corps, or sent to a civil station. He requires to pass an examination in colloquial Hindustani, before he can draw any staff allowance.

With European corps, in which, from the habits of the soldiery and other causes, much sickness usually prevails, he has a good opportunity of becoming acquainted with tropical disease, and of the mode of treatment adopted by men of experience. In native regiments, the practice is usually unsatisfactory, and little professional skill can be attained. In civil stations, the surgeon is generally thrown entirely upon his own mental resources, has charge of a jail and dispensary, attends the European and native officials and their families, and in many stations may obtain a lucrative practice among the neighbouring planters. If possessed of ability and energy, his opportunities of obtaining professional experience are considerable, particularly in operative surgery. Some assistant-surgeons hold lucrative appointments; one is now surgeon to the Governor-General, upon a salary of 1440*l.* a-year; another obtained the office of residency surgeon at Lucknow as a reward for distinguished service in Burmah, and his emoluments cannot be far short of 2000*l.* a-year. None of these appointments are much less than 400*l.* a-year, and some range between this and 1200*l.* annually. Some are professors in the medical college, and in the enjoyment of lucrative private practice in addition.

In Madras, the assistant-surgeons really undergo a sort of professional probation before they are entrusted with independent charges, and in other respects are very much in the same position as in Bengal, except that the prizes are neither so numerous, nor so valuable.

The only distinction in the Bombay service is that most of the assistant-surgeons serve for two years in the Indian navy.

While we have freely animadverted on the senior appointments of the service, we are glad to acknowledge that, upon the whole, there is no country in the world in which the younger members of the profession enjoy greater opportunities of advancement, by ability and exertion, than they do in India. Real merit is seldom overlooked, and the paths in which they may seek reputation are many and varied.

The charge of regiments of irregular cavalry, detached troops of horse artillery, contingent corps of all arms, sanitary stations, and assistant opium agencies, assay offices, all of which are more or less considered as prizes, fall to them.

Their advantages over their brethren in the Queen's army are many, and in no particular more than in the independent action which they attain at an early age. For the full development of the character, and usefulness, nothing can be of greater service to an individual with a well-trained mind, habits of industry, and the determination to succeed, which is so often the key to success.

The interval of promotion in the Indian army is much too long. It averages from ten years—a very rare event—to fifteen or more, so that an officer may thus be nearly forty years of age before he attains the rank of captain, as many assistant-surgeons do not enter the service before they are twenty-four or twenty-five years of age. We have seen that before that time he may have attained the rank of a general of brigade in the French service.

The surgeons of all the three armies are in very nearly the same position as regards pay and other advantages; but, as usual, the greatest number of prizes is in Bengal. Six civil surgeoncies of large cities, all of them possessing some private practice, in addition to four garrison surgeoncies, the office of apothecary, presidency surgeoncies, and a marine surgeoncy, are theirs. There is also a surgeoncy of the general hospital in Calcutta, at present held by the patriarch of the service, and one of the most distinguished members of the profession who ever went to India; but he has long been incapable of performing any duty.

In Madras there are district surgeoncies, and other appointments of some value; and Bombay has a fair quota of good things for its full surgeons.

The value of few surgeoncies is below 800*l.* a year, and with the emoluments of private practice at the Presidency, as the capital is called, some are worth from 3000*l.* to 5000*l.* annually.

In the purely military branches, the most valuable surgeoncies are those of the brigades of horse artillery, European regiments, and light cavalry corps.

The office of superintendent of the Botanical Garden in Calcutta is held by a full surgeon, upon a consolidated salary of 1800*l.* a-year, with a handsome house rent free, and a princely establishment. For this he has, in addition, to perform the duties of Professor of Botany in the Medical College.

The garden at Saharunpore, with the tea plantations in the Dhera Dhoon, is superintended by another surgeon, upon a lower scale of remuneration; it is, nevertheless, a valuable appointment.

The superintendent of teak forests in Burmah is a third surgeon, who enjoys an income of about 1200*l.* a-year, with travelling allowances. In the Madras and Bombay Presidencies are gardens under the care of surgeons on those establishments.

From the above it will be perceived that the prizes thrown open to the profession by the competition for medical appointments in the Indian service, are neither few nor unimportant.

As we are writing for readers in Europe, where little accurate information in matters relating to India exists, we make no apology for introducing at length the following quotation from a pamphlet which has recently appeared in Calcutta.* It is entitled 'Notes on the Condition

* As we have not Dr. Macpherson's pamphlet at hand to refer to, our extract is taken from a paper in No. 45 of the Calcutta Review, entitled, "Surgeons in India: Past and Present," written in an excellent spirit, by one evidently well acquainted with the subject. With all its trials and privations, its exile and banishment, and the difficulties attendant upon scientific research in a tropical climate, there is much to render an Indian home desirable. There is nothing of the struggle with poverty associated with its early trials, that destroys so many promising aspirants at home; and for those who survive the chequered chances of an Indian career, there is always a handsome provision for an old age of honourable retirement.

of the Indian Medical Services,' and is written by Dr. John Macpherson, the acting Secretary to the Medical Board; its accuracy may, therefore, be relied on.

"The medical service of Bombay consists of thirty-five surgeons and one hundred and five assistant surgeons, making a total of one hundred and fifty-eight, with usually a certain number of supernumeraries, never exceeding twenty in number, and generally falling short of that number. It may be said in a general way, that about seventy officers are employed with the army, or in the Indian navy, about fifty are on staff or civil employment, and the large number of thirty-two on leave or furlough, almost all of them on sick certificate.

"The zillah or civil stations appear to be about sixteen in number, and their pay seems to be much the same as that of similar appointments in Bengal, 360 rupees a month, sometimes with an additional hundred for the charge of civil or insane hospitals, or the same sum for a duty which is never assigned to them in Bengal—that of assistant magistrates; and which, since commencing this article, we find has been withdrawn from all assistant-surgeons; they appear to be scarcely ever post-masters or registrars of deeds. There are also in civil or district employ, four superintending vaccinators, each receiving 350 rupees a month besides their military pay. . . .

"The proportion of staff appointments at the Presidency itself, is, as compared with the other Presidencies, large: including the members of the medical board, they amount to about twenty, or almost as many as in the much larger Presidency of Bengal. There are three members of the board, and its secretary, a superintending surgeon, five professors of the medical college, a store-keeper, a surgeon to the General Hospital, and an assistant, a garrison surgeon and assistant, a surgeon of the marine battalion, a civil surgeon and assistant, a police surgeon and assistant, port surgeon, &c., surgeon to the Jansetjee Jeejeebhoy Hospital, an oculist: generally one officer holds two or more of these appointments. . . .

"Most assistant-surgeons in the earlier part of their career are made to serve for two years in the Indian navy. If their stay be not too long protracted in it, there is very little hardship in this, as they have the opportunity of seeing, in fine vessels, a good deal of the coasts of India, Persia, Arabia, &c.; the pay is small, somewhat larger we believe than that for doing duty with a regiment; but as the temptations to extravagance, and even the opportunities of spending money, are few, it may be considered ample. . . .

"Staff appointments at the Presidency are as much sought after at Bombay as in Calcutta, and several of the officers at the Presidency enjoy pretty good incomes from practice, as also do one or two private practitioners. A good deal of the practice lies among the Parsees, who are, after the Europeans, the leading class, and certainly the most intelligent and enterprising of Orientals. They are, however, much in the habit of employing private practitioners. The best medical practice is not nearly so remunerative as in the larger city of Calcutta, nor even equal to Madras. . . .

"The Madras medical service consists of about seventy-two surgeons and one hundred and fifty-four assistant-surgeons, making a total of two hundred and twenty-six. Of these some eighty are on staff employ, ninety in regimental employ, and some fifty absent on sick or other leave.

"Of those on staff employ, about twenty-eight are zillah surgeons, ten employed in residencies. The pay of zillah surgeons is, we believe, the same as in Bengal and Bombay. We should suppose that civil surgeoncies, generally speaking, cannot be very remunerative. We have heard of Salem as a good civil station. The Neilgherries must yield a considerable income, and of the surgeoncies, Hyderabad is of course the best, rivalling Lucknow; the surgeoncy to the Mysore commission and some other residency surgeoncies, as that of Cochin, are comfortable appointments. There are ten officers in the Nizam's service, all well paid, five garrison surgeons, no fewer than ten superintending surgeons, three members

of the medical board, and a secretary, the latter at present being an assistant-surgeon.

"There are at the Presidency fifteen medical officers, including the medical board, being eight surgeons and seven assistant-surgeons. Their duties are those of garrison surgeon, medical store-keeper, four district surgeons, superintendent Eye Infirmary, surgeon General Hospital, one permanent assistant, and one assistant-surgeon to it, and six chairs in the Medical College. . . .

"Assistant-surgeons in Madras are, on their first arrival, made to do duty at the General Hospital, and keep case-books, until they are reported, fully qualified for the general duties of the Army. . . .

"The Bengal medical service consists of a hundred and twenty-nine surgeons and two hundred and thirty assistant-surgeons, making a total of three hundred and fifty-nine. There are supposed to be a certain number of supernumeraries attached, but this is by no means always the case. The service may be said generally to be divided into two hundred employed in purely regimental duty (including irregular cavalry and local corps), about a hundred and twenty on civil or staff employ, and forty on furlough or leave, the proportion of the latter being much smaller than in Madras, and little more than one-third that of Bombay,—a very remarkable fact! We may here remark that it is a subject of some just complaint in Bengal, where promotion is so slow, that a surgeon of thirty years' service, when on furlough, draws no higher pay than one just promoted, but this is also the case with the captain who is unlucky in his promotion.

"Some of the chief civil and staff appointments, besides the eleven superintending surgeons and the members of the board, are the following: viz., upwards of fifty civil stations in Bengal and the north-west; of these only six are assigned to full surgeons, namely, Delhi, Agra, Benares, Patna, Dacca, Berhampore, and are all more or less sought after. Of the appointments in the north-west the most lucrative for assistant-surgeons are Bareilly, and the civil surgeoncies of Simla and Mussoorie: the two last only held for a period of two years. In Bengal there are many very excellent civil appointments, supposed to vary in value from 700*l.* to 1100*l.* a-year, as Kishnaghur, Howrah, Jessore, Tirhoot, Bhaugulpore, Chuprahi, Hooghly, Ghazee-pore, &c. The registry of deeds is in some of the cases the most valuable addition to the appointment, while in Kishnaghur the ferry gives a handsome return, but in all these cases the value of the appointment depends much on the popularity of the civil surgeon with the station and the neighbouring planters and landholders. In Bengal and the north-west the civil surgeons very generally hold the post-office, and are also often registers of deeds. But under the new changes they are to be deprived of the post-offices, and the civilians always endeavour to get hold of the registries for themselves.

"Of political appointments, strictly speaking, only two are now held by members of the medical service, namely, the charge of Darjeeling, and the custody of the young Maharaja. The two chief residency surgeoncies are excellent appointments, Lucknow being worth 1500 rupees a month to any one of common judgment, and Nagpore about 1200 rupees. The superintending surgeoncy at Gwalior is a desirable appointment, as indeed must the charges in the Gwalior contingent generally be considered. The opium examinations at Ghazee-pore and Patna are excellent appointments; that at Indore is now held by a Bombay assistant-surgeon. Two mint appointments, one in Bombay and one in Calcutta, are held by Bengal surgeons, as well as the charge of the botanical gardens in Calcutta and Saharan-pore; one of the examinations of the College of Fort William is also held by a medical man.

"Including the medical board, there are twenty-five members of the medical service performing medical duties at the Presidency, and this is including the surgeon to the Governor-General and to the body-guard, who cannot be looked on as fixtures. In the last ten years the offices of second garrison-assistant, deputy apothecary, and marine assistant-surgeon, have been abolished, and one Presidency surgeoncy absorbed in the marine surgeoncy. Some of the appointments at the

Presidency are the medical store-keeper or apothecary, the garrison surgeon and assistant, the surgeon to the General Hospital and his two assistants, the marine surgeon, the oculist, six or seven professorships in the Medical College, and five Presidency surgeoncies.

A peculiar feature of the Indian medical services, in which they bear some resemblance to the medical department of the French army, is that they furnish the instructive staff for four medical schools—viz., the Colleges of Bengal, Madras, and Bombay, and a school of more restricted dimensions established in the Nizam's territory. The former are highly-organized institutions, have been eminently successful in their results, and are justly held in high esteem by the Government and the public. They have done much to overcome prejudices of caste, have proved that the natives of India are susceptible of a high degree of moral and intellectual training, and in their systems of examination for diplomas are far in advance of all examining bodies in Great Britain, with the exception of the University of London. The practical tests, in particular, are so extended and severe, as to render it impossible for men to be sent abroad to practice in the country, who have not been in diligent attendance upon hospitals and dissecting-rooms during their pupilage. Cramming and grinding are unknown in those schools.*

It would obviously be out of place in this paper to discuss the subject of the means adopted by the various bodies licensed in the United Kingdom, to ascertain the qualifications of those who present themselves for the degrees and diplomas necessary for the killing or curing of her Majesty's lieges, *secundum artem*. It is inseparably connected with the still more important matter of medical education itself—a subject of the greatest importance to the well-being of the profession.

It is sufficient for our purpose to declare our belief, that the possession of a degree or diploma from any examining body in Great Britain and Ireland, except perhaps the University of London, is not of itself a proof

* The subject of medical education in India is too important to be discussed as an incident in remarks devoted to another purpose. Should no one better qualified to do justice to the task undertake it, we shall esteem it a pleasure to introduce the matter to European readers at some future period. As a link in the chain of human progress—as an evidence of the earnest and honest manner in which the character of the profession is upheld in the far East—and as a type of the mode in which the civilization of the West is beginning to leaven the masses in India, it will, we doubt not, recommend itself to the sympathies of our brethren in Europe.

There is probably no department of the public service in India that has produced a greater proportion of distinguished men than the medical fraternity. To the patriotism of two of their number, Boughton and Hamilton, the foundation of the empire which is now the brightest jewel in the British Crown, may fairly be traced. The professional skill of the former, successfully exercised upon the daughter of Shah Jehan, procured the long-desired privilege of establishing trading factories in Bengal, free from taxation. When this privilege was on the point of expiring, from causes which are well known to all readers of the history of British dominion in the East, it was renewed by the surgical success of the latter upon the person of the Emperor Furkshere. Both of these men died in the land of their adoption, an honour to their country and their profession.

To Oriental and general literature they have contributed some of the greatest names in the annals of the Eastern Empire; and there is no department of usefulness to which they have not contributed a fair quota of men of eminence and ability.

To one of their number India owes the successful introduction of the lightning which transmits intelligence with the swiftness of the passage of light. To others, working with equal skill and ability in a still higher path of human exertion, is, in some degree, due the rapid progress of education, which, like a mighty and irresistible torrent, is sweeping away, in the current of knowledge and truth, the superstition of ages, and restoring to the East the light of civilization, with which she originally dispelled the Cimmerian darkness of the West.

of the fitness of the possessor to be entrusted with the care of the life and limb of the soldier, or of the communities in India who are entirely dependent upon their skill in the dread hour of disease and suffering. So long as such degrees and diplomas are procurable by mere book knowledge, or the objectionable system of preparation known under the term of "*grinding*," there can be no security against the introduction into the profession of men altogether unequal to the emergencies liable to arise in the course of practice.

This is of little comparative consequence in the civil practice of Europe, where the beneficial effects of competition generally correct any mischief that might result from entire ignorance or incapacity. The public are free to choose their medical attendants, and it is their own fault if they risk their lives in the hands of the unskilled. To be sure there may be some difficulty in the selection, for presumption and ignorance usually go hand in hand, and no man of character and capacity can venture to proclaim the infallibility to which empiricism pretends.

The soldier has no choice in his medical attendants, and must submit to whatever the Government provides for him. It is lamentable to reflect upon the amount of mischief that must ensue from incompetence in a military surgeon. Two examples, among several that have occurred within our own knowledge, may be mentioned in proof of the extreme care that should be exercised in such appointments.

A large amount of sickness and mortality prevailed in a European corps in India some years since, and the principal inspecting officer was directed by the Government to proceed to the station, to ascertain personally, the cause of so costly a loss to the State. When he arrived at his station, he proceeded at once to the quarters of the surgeon, whom he found dead in his chair, with the inspector's letter lying open on the table before him. He was unequal to the performance of the duties of his office, and ended his sorrows with a dose of morphia, to avoid the shame of exposure.

The other example happened some fifteen years since, in a small field-force, of which an assistant-surgeon was in medical charge. In the first encounter with the enemy, an artilleryman was so severely hurt as to require the immediate amputation of a limb. The surgeon entered his tent, ostensibly to procure his instruments; but, as he was rather long absent, an inquiry was instituted to know what had become of him. He was found lifeless. He had committed suicide to avoid the performance of an operation with which he was practically entirely unacquainted.

Such things are merely mentioned to impress the fact that more care is necessary in regard to the practical tests in the case of officers selected for service in India, than in ordinary cases.

The recent regulations which have thrown open the medical services of India to the profession at large, will, when fairly carried out, secure for Hindustan a similar quality of professional acquirement as the system of *concours* in France has secured for the Polytechnic School.

We look upon the institution of the principle of the *concours* on the large scale, now for the first time practically introduced in England, with the greatest interest, and are convinced that it is fraught with much that is of the most vital importance to the well-being of the profession at home

and abroad. At the same time, let us pay a tribute of gratitude to the East India Company, in whose hands this great patronage has existed for so many years, and by whom it has been exercised with a purity and honesty utterly unknown to the political distribution of patronage in England. That eminent body has been the kindest, most liberal, and considerate government in existence towards its servants; and its service is justly esteemed the best of the whole world by those who have passed their best days in it, and have seen how their less fortunate brethren are treated elsewhere.

All medical officers in the Indian armies are entitled to retire, without reference to the state of their health, at fixed periods, commencing from seventeen years' service and extending to thirty-five, the pension from the State varying from 200*l.* to 750*l.* a year, at the extremes of the scale.

In addition to this, they subscribe to funds somewhat similar in effect to the purchase of annuities for the remainder of their lives, and to which the Court of Directors contribute handsomely. In Bengal, this amounts to an addition of 300*l.* annually to the income of the retired surgeon, of which the subscriber must pay a minimum of its half value, as calculated upon his age at the time of retirement. It is now obtained after an average service of twenty-four years.

In Madras, the fund is administered with some differences of detail. At the end of seventeen years' service (subject, however, to the state of the fund), the medical officer is entitled to a certain allowance, amounting to 200*l.* per annum; and after a certain additional number of years, spent either in the service or not, he becomes entitled to a further increase of 200*l.* per annum. It is usually, we believe, some six or eight years before an officer who has received the smaller, becomes entitled to the larger, allowance. Occasionally, when there have been many claimants upon the fund, officers wait till they have put in twenty, or even twenty-two or twenty-three years' service, before they can obtain even the small allowance.

The retiring allowance from the Bombay fund is, we believe, administered on the same principles as that of Bengal, but we think its allowances are not quite so large.

For his widow and orphans, should the medical officer die before he has been able to provide for them, and even after his retirement from active service, a handsome provision is made, chiefly at his own expense, but again with reasonable help from his rulers.

In all these respects, how superior is his treatment to that of his compeers in the service of Her Majesty, and to those of any class of public functionaries on the Continent of Europe!

An invaluable appendage of the Indian army is the subordinate medical department attached to it. This, in Bengal, consists of the European establishment, and of a special class of subordinate agency for the native army, and for duty in civil hospitals appropriated to natives.

The European establishment consists of apothecaries, assistant-apothecaries, apprentices, stewards, and assistant-stewards.

The apothecaries are charged with the preparation and administration of medicines, the care of wounds, accidents, and injuries, during the

intervals of the visits of the surgeons, the admission of patients, and, in fact, are the general assistants of the medical officers in the performance of their professional duties in the field, in garrison, and in all the circumstances in which the troops are employed. It would be impossible to exaggerate the usefulness and importance of this excellent class of public servants. As a body, they are a credit to the service, and are of more real use, from their careful professional training, than any body of nurses could possibly be, to the sick and wounded.

They are usually the sons of soldiers, educated in the regimental schools, or in the Military Orphan School. They are admitted to the service after examination by special committees of medical officers—a *concours* upon a small scale—and after doing duty in regimental hospitals for two years, are (if in Bengal) transferred to the medical college in Calcutta for two additional years of training. There they are under strict military control; are instructed in anatomy, materia medica, medicine, and surgery; are carefully trained in hospital duties as clinical clerks; and, after undergoing a tolerably strict examination—in some particulars more severe than that of the College of Surgeons of England—are reported qualified. If they fail, are idle and insubordinate, and otherwise misconduct themselves, they are removed from the army, and forfeit all the advantages of their previous service.

In the recent Burmese campaign, and in the late Punjaub war, they were found most efficient field-assistants; and we are able, from personal knowledge, to state that some of them are more efficient members of the profession, and generally better informed, than some assistant-surgeons with whom we have come in contact, armed with degrees and diplomas from British schools of old and great pretensions.

Now that the rank of apothecary in the British army has, like that of the French service, been made that of a commissioned officer, we trust that the Court of Directors and Board of Control will extend a similar boon to the most deserving members of this invaluable class of public servants. Their length of service, their high personal character, and their qualifications, fairly entitle them to such a mark of consideration.

If so great an effort is made at home to reward the private soldier for distinguished conduct in the ranks, to raise him in the estimation of his country, and to encourage a better class of men to enter the army, there is no reason why the boon should be limited to any particular class of the soldiery.

We are acquainted with some members of the European medical subordinate department in India, who would confer greater honour upon a commission, than the parchment patent could possibly confer upon them. Had such a department, so organized and instructed, existed at the outbreak of hostilities with Russia, some of the most serious charges against the medical department, and most of the painful details which have harrowed the feelings of the public, would not have been heard of,—for they could, under such a system, have had no existence.

The stewards and their assistants are charged with all the details relating to the food, clothing, and similar interior economy of military hospitals. Both classes aid the surgeon in the preparation of official reports and statements.

These documents are much too cumbrous and complicated, employ a most unnecessary portion of the surgeon's attention, in times of severe sickness, in the field, and in all emergencies, and frequently convert him into a species of professional clerk. As checks, they are too complicated to be effective—as records, they are too voluminous for examination. A sensible sanitary commission, such as we hope, ere long, to see established in both armies, would soon reduce them to convenient working dimensions, and extract from them invaluable matter for future guidance. At present, they are *caviare* to everyone connected with the medical services in India and in England.

In the native army, and in the ordinary civil hospitals of the country, there is a special class of indigenous subordinates, somewhat quaintly termed Native Doctors. They are instructed in the medical college, in their own vernacular language, in anatomy, materia medica, medicine, and surgery, taught by their own countrymen previously educated in the English class of the same college. They are not so highly qualified as the European subordinate class, but are nevertheless an extremely useful body of men.

Another feature peculiar to the Indian system is the existence of great store departments in the three Presidencies, whence issue all the medicines, instruments, and other necessities of military and civil hospitals. The establishment of such magazines was doubtless rendered necessary, in the first instance, by the great distance of the country from the central source of supply, the uncertainty and length of communications with the mother country, and the impossibility of procuring what was required on the spot.

In Bengal, the principal medical store-keeper is termed apothecary to the Company, and, under the authority of the medical board, regulates the issue of all medicines, instruments, and apparatus, and indents upon Europe for all future supplies. All indents are forwarded through the superintending surgeon of the division, by whom they are, in the first instance, checked and countersigned. They then pass through the medical board, and, after undergoing further scrutiny, are sent to the apothecary to supply. They are prepared and packed with the greatest care and attention, and, in cases of urgency, with the least possible delay.

All stores and instruments arriving from Europe are examined and passed by a committee, selected in rotation from the medical officers at the Presidency. All remarks made by this dispensary committee are forwarded to the medical board, and transmitted to Europe with the next succeeding annual indents, should they contain suggestions for improvement, or objections to the nature and quality of the medicines and instruments forwarded by the Court of Directors.

There are branch depôts in the North West Provinces, to save the time and trouble involved in references to Calcutta, and to enable the frontier stations to be supplied without delay.

When an army takes the field, a special store-keeper, generally an active assistant-surgeon, is appointed to take charge of the medicines and instruments for issue to field hospitals. In addition to the wants of each regiment about to proceed on service being fully supplied, a calculation is made of the probable requirements of the field hospitals, with a large

margin for contingencies. In this manner a force of 100,000 men can be supplied as readily as a division of 10,000; and it is no fault of the Government if there is any deficiency of the means and appliances required by the exigencies of any campaign, with its attendant casualties.

In Indian warfare, disease is, if possible, more destructive than in Europe, although even in the latter the loss by the sword, and from actual contact with the enemy, is seldom one-fourth of the entire loss sustained. In the Peninsula campaign, out of 40,000 dead, less than 10,000 fell in action or died of their wounds. Such exceptional cases as Walcheren, the retreat from Moscow, the Cabul disaster, and, unfortunately, the Crimean expedition, cannot enter into such a calculation, as they are wholesale destructions of life beyond the average of ordinary warfare. This exhibits most strongly the necessity for having magazines well supplied to meet urgent demands, as time is of the utmost consequence in such cases, and the early supply of medicines in sufficient quantities, may be the means of saving hundreds of valuable lives. It is unsafe to depend upon the market, or upon ordinary sources of supply, in time of war, for reasons too obvious to need any detailed demonstration.

Such is, we believe, a faithful outline of the constitution and principal peculiarities of the Indian Medical Service. We have purposely, in all our remarks, avoided minute details; our object being rather to present a general view of the subject, than to exhaust any portion of it.

In the present great crisis of public affairs, we deem it of the utmost importance to make known, as widely as possible, the state of our medical departments, that the remedies for the removal of the evils surrounding them may be applied rapidly and efficiently.

England has been taunted with the absence of the administrative ability necessary to organization, and to accomplish great ends with the means at her command. We can proudly point to India to disprove the assertion, and to show that the talent exists in abundance, if the right means are sought to turn it to advantage. There, the mightiest results have been accomplished in circumstances sometimes apparently as hopeless as the present paralysis of our arms in the Crimea; and when great disasters have occurred in India, they have been due to the incapacity of the chiefs entrusted with the guidance of affairs—in no instances to the unfitness of the instruments at their disposal.

And now, let us turn, in conclusion, to the means necessary to place the Indian medical establishments upon the most efficient footing, and to the rewards necessary to encourage first-rate men to enter the service of the East India Company, and to remain in it as long as they are capable of doing good service. To beat up for recruits by public competition, and then to consign them to oblivion when fairly caught, is neither prudent nor politic. If the means of fostering merit, rewarding acknowledged zeal, and rapidly advancing men of more than ordinary capacity, are not adopted, the system of competition will fail, and a secondary class of men only will enter the lists. The able and zealous are sure to succeed, wherever their lot is cast; and they will certainly not subject themselves to the chances of a contest which leads to no honour. Hence the present is, we believe, the most favourable juncture for a radical change in the present constitution of the medical departments of the Indian

army. It is manifestly more difficult to introduce fundamental changes into a service, the existing members of which have entered it upon an implied guarantee of necessarily rising by seniority, during good conduct, than into the Queen's army, where no such system prevails, beyond that which obtains in all services, viz., that length of service, combined with fitness, constitutes strong claims to advancement.

In the Indian army this really applies only to promotion to a surgeoncy, and to pensions for specified periods of service, which it would manifestly be a breach of faith to disturb. The higher offices in the hierarchy have long been declared by authority to be *staff appointments*, and, as such, to be selective. To make them so absolutely, will only be to reduce the written law to practice.

The first and most important change required is to place the department under a single responsible head, selected from the whole service, in direct communication with the Government, and possessed of authority to enforce obedience to all departmental instructions. At present not only is the medical service imperfectly represented, but its representations are distilled through other channels in which they may be, and frequently are, diluted and destroyed, before they reach the head of the Government. There are a thousand ways in which this operates injuriously.

With regard to the members of the service generally, the smallest boon that can be extended to them is to assimilate their state to that of the Queen's army, in the matter of establishing staff surgeoncies of the first class, and in the more rapid promotion of assistant surgeons. No officer should remain in that class, in any circumstance, longer than ten years; and, if possible, men of distinguished merit should be able, as in the French army, to run through all the ranks of the hierarchy in fifteen years.

The present injurious distinction in regard to dress should be abolished, and a departmental uniform adopted, which no man need be ashamed to wear. At present, we doubt if any medical officer would, if he could avoid it, ever appear in the full dress of his rank. What motive can have suggested the personal degradation of an entire class in such a matter, we have never been able to divine.

Every medical officer, particularly in the Indian army, should, in our opinion, be mounted. It would cost nothing additional to the State, and, in the field, would undoubtedly increase his usefulness.

The Indian army should be provided with a proper ambulance corps, organized and equipped in a manner suitable for service in Asia.

There should be established a sanitary commission to regulate and report upon all matters connected with the health of the soldier, and the health of the general community in India.

The important subjects of registration, statistics, vaccination, and, in fact, everything relating to public health, in the most extended sense, should be confided to its care. It should consist of at least three members, selected, without reference to rank and standing, from the whole service, should be liberally paid, and act directly under the head of the medical department.

The labours of such a commission would in a few years far more than repay its cost to the State, in the improved economy of hospitals, jails, and

similar institutions, and in devising the most suitable means of diminishing the preventable causes of disease, which the present advanced state of medicine, as a science, enables us to anticipate with confidence.

This is the merest sketch of the principle which should guide improvements in the Indian army. It would be easy to work out such a scheme in detail, were it advisable in the present instance to do so.

With regard to honours, if it be deemed not desirable to bestow an occasional baronetcy or knighthood on any officer of very superior and decided merit, upon the recommendation of the Governor-General of India, the honours of the Bath should be opened to the military surgeon for service in the field, as freely as to other branches of the army.

For distinguished services in the civil ranks of the profession, it would not be difficult to devise a suitable badge of distinction, upon the principle of the corresponding branch of the Legion of Honour in France. Ribbons and crosses are the cheap rewards of governments. "The reward of individual services," says a recent writer on this subject, "is an important part of the debt due by the State to its officers; but its importance does not end here. For one individual marked out for reward and distinction, we make a hundred others envious of similar rewards, and eager, through the exercise of their talents, and the amount of their services, to deserve similar recompences."

REVIEW XIII.

Mode of Communication of Cholera. By JOHN SNOW, M.D.

Second Edition.—London, 1855. pp. 162.

NONE of our readers can be ignorant of the opinions of Dr. Snow on the communication of cholera by means of drinking water, nor of the perseverance and energy with which he has sought for facts to corroborate this view. The present work is a recapitulation of all the evidence he has hitherto published, with the addition of certain facts lately acquired.

We have read this work carefully, and shall endeavour, in the following critique upon it, to do full justice to Dr. Snow, while we shall strictly examine, as it is our duty to do, if there is anything hollow or unsound in the facts brought forward, or in the arguments founded upon them.

Dr. Snow believes not only that cholera is propagated by means of water, but that it is solely and exclusively so propagated. He is therefore obliged, at the very outset of his inquiry, to assume that cholera only spreads where human intercourse is possible. Thus he writes,

"It travels along the great tracks of human intercourse, never going faster than people travel, and generally much more slowly. In extending to a fresh island or continent, it always appears first at a sea-port. It never attacks the crews of ships going from a country free from cholera, to one where the disease is prevailing, till they have entered a port, or had intercourse with the shore. Its exact progress from town to town cannot always be traced; but it has never appeared except where there has been ample opportunity for it to be conveyed by human intercourse." (p. 2.)

We do not wish to argue the several clauses of this paragraph, but we do most decidedly protest against its reception, as a complete and final expression of the mode of spread of cholera. While we admit at once

that there are now several cases which show human intercourse to be occasionally influential in some way in transmitting cholera, we deny altogether that the phenomena, either of its rise or decline, can be always, or even frequently, so explained.

But our object now is not to discuss the general question of the contagion of cholera, but to see the strength of Dr. Snow's evidence on one particular presumed mode of propagation.

At page¹⁰, Dr. Snow informs us that he was led to his view of the spread of cholera by a consideration of its pathology. He believes that the symptoms commence in the intestinal canal, and (apparently) that the disease is entirely local, and that neither the blood nor the nervous system are primarily affected. Having arrived at this opinion on grounds which appear to us insufficient to warrant so grave a conclusion, he writes as follows:*

"As cholera commences with an affection of the alimentary canal, and as we have seen that the blood is not under the influence of any poison in the early stages of this disease, it follows that the morbid material producing cholera must be introduced into the alimentary canal—must, in fact, be swallowed accidentally, for persons would not take it intentionally; and the increase of the morbid material, or cholera poison, must take place in the interior of the stomach and bowels." (p. 15.)

We cannot admit the cogency of the *must* in this quotation; since we do not see that it is satisfactorily made out that the blood is "not under the influence of a poison."

Dr. Snow then goes on with the following sentence, for which we can find no warrant in anything which has gone before; it appears to us to be a mere hypothesis.

"It would seem that the cholera poison, when reproduced in sufficient quantity, acts as an irritant on the surface of the stomach and intestines, or, what is still more probable, it withdraws fluid from the blood circulating in the capillaries, by a power analogous to that by which the epithelial cells of the various organs abstract the different secretions in the healthy body." (p. 15.)

Dr. Snow afterwards says, that there is sufficient "to prove the communication of cholera here explained, independently of the pathology of the disease." (p. 16.) We do not hesitate to say that the argument from pathology seems of little value, and it somewhat lessens our faith in Dr. Snow, to find that so doubtful a view, to say the least of it, should have suggested a special opinion of the propagation of the disease.

However, this is of little moment, and we proceed to the grand object of the work, and of this review—viz., the evidence for the propagation by water, and an examination of it.

From page 16 to page 22, Dr. Snow makes general reflections on the habits of the persons among whom cholera chiefly prevails, in order to show that their want of cleanliness would render it likely that portions of cholera evacuations might get mixed with their food or drinking water. We pass over this as not of great weight, and arrive, at page 22, at the first page in which direct evidence is sought to be given in favour of the hypothesis.

1. In two small adjacent courts in Horsleydown (Surrey-buildings and

* In this and in subsequent extracts we have italicized some passages which contain the pith of the sentence, or which appear to us illogical.

Truscott's-court), cholera prevailed (in 1849) with very unequal severity, 11 fatal cases occurring in Surrey-buildings, and 2 cases (1 fatal) in Truscott's-court. In all other respects similar (according to Dr. Snow), the only difference was in the water supply; the well supplying Surrey-court was contaminated by the slops and dirty water in which the clothes were washed being poured by the inhabitants into drains which communicated with the well; while the water of Truscott's-court was free from this impregnation. A curious fact was, that all the fatal cases were in women and children.

Admitting Dr. Snow's hypothesis would explain all the other cases, we must account in some different way for the first case. The mode in which this is done shows Dr. Snow's perfect faith in his creed; instead of leaving the origin of the first case uncertain, he says the "two first cases were probably caused by the cholera evacuations contained in the Thames water, as it came from the waterworks." (p. 24.) Considering that this is the very point to be proved, we think the probability of this alleged mode is rather too easily admitted.

We visited these courts in 1849, not long after Dr. Snow's visit, and found that the number of the population of Truscott's-court was very much below that of Surrey-buildings. We have not been able to find the note made on the point, but we distinctly remember that the disproportionate mortality in the two courts was in this way partly, though perhaps not altogether, explained.

We again visited these courts a few weeks ago, and found the differences between them to be as great as we had conceived them to be. Thus, Surrey-buildings consists of fifteen houses with four rooms each, while Truscott's-court contains only ten houses with two rooms each. There are, therefore, sixty rooms in the first-named court to twenty in the second; and if we suppose the density of population to be two to each room, there would be 120 persons in Surrey-buildings to 40 in Truscott's court; and if the cholera had been equally severe in the two courts, it would of course have affected three persons in one, to one in the other. The disproportion is thus partly, but not altogether, removed. Truscott's-court, however, with its small, low houses, is much more airy than its neighbour; it is close to an unoccupied piece of ground, of the benefit of which Surrey-buildings, facing the other way, and having no through ventilation, is deprived. We do not think that any one seeing these two courts, and knowing how much accumulation of persons in the same house tends to aggravate cholera, and how ventilation checks it, but would see in these circumstances an explanation, as satisfactory as that of contaminated water, of the difference in the mortality which remains unexplained by the difference in population.

2. The next case is that of Albion-terrace, Wandsworth, where, in 1849, cholera prevailed in a single row with considerable severity. Dr. Snow informs us that

"The water got contaminated by the contents of the house drains and cesspools. The cholera extended to nearly all the houses in which the water was thus tainted, and to no others.

"These houses were numbered from 1 to 17, in Albion-terrace, and were supplied with water from a copious spring in the road in front of the terrace, the

water of which was conducted, by a brick barrel-drain between Nos. 7 and 8, to the back of the houses, and then flowed right and left, to supply tanks in the ground behind each house, the tanks being made of brickwork and cement, covered with a flat stone, and connected with each other by stoneware pipes six inches in diameter. A leaden pipe conveyed water from each tank to a pump situated in the back kitchen. There was a cesspool behind each house, under the privy, and situated four feet from the water-tank. The ground was opened behind the houses No. 1 and No. 7, and the drains examined under the superintendence of Mr. Grant, the assistant-surveyor to the Commissioners of Sewers. The cesspools at both these places were quite full, and the overflow-drain from that at No. 1 choked up. At this house the respective levels of the cesspool and the water-tank were measured, and the top of the overflow-drain from the cesspool was found to be fifteen inches above the top of the tank, and the intervening ground was very wet. The overflow-drain mentioned above had no bottom, or one so soft that it could be penetrated with a stick; and it crossed, at right angles, above the earthenware pipe of the water-tank, the joints of which were leaky, and allowed the water to escape. Behind No. 7, Mr. Grant found a pipe for bringing surplus water from the tanks, communicating with a drain from the cesspool; and he found a flat brick drain laid over the barrel-drain before mentioned, which brought the water from the spring. It appears, from a plan of the property, that this drain, which was continued in a direction towards the sewer in Battersea-fields, brought surface-drainage from the road, and received the drains from the cesspools, the house-drains from the sinks in the back kitchens, and the surplus water, or some of it, from the tanks. There was every reason to believe that this drain was stopped up, but that was not ascertained: at all events, it was unable to convey the water flowing into it during the storm on July 26th, as it burst near the house No. 8, and inundated the lower premises of that and the adjoining house, No. 9, with fetid water; and it was from this time that the water, which had occasionally been complained of before, was found by most of the people in these seventeen houses to be more or less impure or disagreeable." (pp. 25-7.)

And he gives a description of the drains and water pipes, to explain how this took place.

The first person was attacked on July 28th:

"There are no data for showing how the disease was communicated to the first patient, at No. 13, on July 28th; but it was two or three days afterwards, when the evacuations from this patient must have entered the drains having a communication with the water supplied to all the houses, that other persons were attacked, and in two days more the disease prevailed to an alarming extent." (p. 28.)

The premises were examined by Dr. Milroy, who attributed the outbreak to an open sewer, to effluvia from sinks, and to an accumulation of offensive rubbish, in the house in which the first case occurred. Dr. Snow considers, and perhaps correctly, this explanation to be incorrect.

We must observe, however, that in addition to the impregnation of the water, it appears that there must have been, immediately before the attack of cholera, contamination of the air also, as on the 26th of July, two days before the first case, the lower premises of two houses were flooded with fetid water from the blockage of the drain; and doubtless the same effect was produced, in a less degree, in the other houses. Although we are not informed how long this stagnant water remained, it would probably, for some days, be sufficient to render the air impure. We are led to believe, from Dr. Snow's account, that the water had been previously contaminated by faulty arrangement of the drain and water-

tanks, and yet no decided cholera seems to have occurred till this overflow, two days after which the first case (the origin of which Dr. Snow leaves doubtful) took place.

One fact mentioned by Dr. Snow strengthens our suspicion that the air may have had as much to do with it as the water, for it is mentioned, that

"There were two or three persons attacked with cholera amongst those who came to nurse the patients after the water was condemned, and who, consequently, did not drink it; but these persons were liable, in waiting on the patient, to get a small portion of the evacuations into the stomach in the way first pointed out; and there might be food in the houses previously prepared with the tainted water." (p. 29.)

These conjectures as to how the cholera matter got into the stomach of the nurses who did not drink the water, appear to us to be rather out of place. The point is to prove the fact of water being the agent, and not to assume it, and then to seek for some other explanation of those cases for which the presumed contamination cannot account.

3. Dr. Snow then quotes from Dr. Lloyd two instances in which cholera prevailed in limited districts, where water contaminated with sewage matter was used. The first instance is very inconclusive; the second is the following:

"Charlotte-place, in Rotherhithe, consists of seven houses, the inhabitants of which, excepting those of one house, obtained their water from a ditch communicating with the Thames, and receiving the contents of the privies of all the seven houses. In these houses there were twenty-five cases of cholera, and fourteen deaths; one of the houses had a pump railed off, to which the inhabitants of the other houses had no access, and there was but one case in that house." (p. 31.)

In this example, as in almost all the other cases adduced by Dr. Snow, we miss the very necessary information as to the number of persons resident in each house; their ages, occupations, and habits; the kind of house in which they lived, &c. In six houses there were altogether twenty-four cases of cholera, in the seventh house only one case. For anything we are told to the contrary, however, there may have been only a single case in one of the six houses, and a greater number than the average in some of the others. If this were so, the point and force of the argument at once disappears.

4. Another case, quoted from the Board of Health, is stronger:

"In Manchester, a sudden and violent outbreak of cholera occurred in Hope-street, Salford. The inhabitants used water from a particular pump-well. This well had been repaired, and a sewer which passes within nine inches of the edge of it became accidentally stopped up, and leaked into the well. The inhabitants of thirty houses used the water from this well; among them there occurred nineteen cases of diarrhoea, twenty-six cases of cholera, and twenty-five deaths. The inhabitants of sixty houses in the same immediate neighbourhood used other water; among these there occurred eleven cases of diarrhoea, but not a single case of cholera, nor one death." (p. 31.)

5. A fifth instance is mentioned, on the authority of Dr. T. K. Chambers:

"Dr. Thomas King Chambers informed me, that at Ilford, in Essex, in the summer of 1849, the cholera prevailed very severely in a row of houses a little way from the main part of the town. It had visited every house in the row but one. The refuse which overflowed from the privies and a pigstye could be seen

running into the well over the surface of the ground, and the water was very fetid; yet it was used by the people in all the houses except that which had escaped cholera. That house was inhabited by a woman who took linen to wash, and she, finding that the water gave the linen an offensive smell, paid a person to fetch water for her from the pump in the town, and this water she used for culinary purposes, as well as for washing." (p. 32.)

We think we ought to be careful how far we attach great weight to an argument of this sort; we are not informed how many persons lived in the house with this woman, how many houses there were in the row, and other circumstances of the like kind. If the woman was the only inhabitant of the house, as we are led to conclude from the mode in which the sentence is worded, her escape would have nothing wonderful about it, as only the minority are attacked with cholera.

6. The next case is one mentioned to Dr. Snow by a friend. Cholera prevailed at Locksbrook, near Bath, and the owner of some houses was informed that the water was bad:

"He sent a surveyor, who reported that nothing was the matter. The tenants still complaining, the owner went himself, and on looking at the water and smelling it, he said that he could perceive nothing the matter with it. He was asked if he would taste it, and he drank a glass of it. This occurred on a Wednesday; he went home, was taken ill with the cholera, and died on the Saturday following, there being no cholera in his own neighbourhood at the time." (p. 32.)

Thus the surveyor finds nothing wrong, and the hapless owner is so convinced that the water is good, that he drinks a glass of it, and dies three days afterwards; therefore it was the water.

Nothing could be very much looser than this statement; we might just as well affirm that the man having been in the very place, and in the very houses, where cholera was, received it, as we believe many persons do receive it, through the medium of the air. Dr. Snow will say that the other evidence in proof of the propagation by water gives weight to his view of the case, but we may quite as well say that the evidence by transmission through the air is much more stringent, and gives weight to our hypothesis.

7. The next case appears to us to be quite worthless as evidence. The village of Newburn, near Newcastle-on-Tyne, suffered frightfully from cholera in 1832, at which time the sources of water-supply were the same as at present. Dr. Snow applied to Mr. Davison, surgeon, of Newburn, who informed him that—

"It was considered that the evacuations of the people could not get into any of the wells; but the vicar thought that the water of a little brook which runs past the village, and falls into the Tyne immediately afterwards, might find its way into that well which is chiefly resorted to." (p. 33.)

On further inquiry, it was found that the brook received "the refuse of a small village," and also passed "through a privy used by the workmen of a steel factory." The drain conveying water from an old coal-pit to the well chiefly used in Newburn passed underneath the brook, and then ran alongside it for about three hundred yards:

"Mr. Davison said that it was disputed whether there was any communication between the drain and the brook, but that it was highly probable that there might be; and that an occurrence which took place a few months previously seemed to prove that there was. Some gas-water from the steel manufactory mentioned

above got by accident into the brook, and some of the people affirmed that the water in the well was strongly impregnated with it." (p. 34.)

Thus the water of the brook, it is thought, *might* find its way into the drain leading to the well. Although no communication can be traced between the two, the affirmation of *some* of the people, that the well had been impregnated with gas-water some months before, is taken as conclusive evidence that there was such communication during the time of the cholera: that conclusion arrived at, the first case of cholera occurs; it is left unaccounted for, but from it, it is easy to deduce all the rest.

"As several days elapsed between the first case of cholera and the great outbreak, it is probable that the water in which the soiled linen must have been washed, and which would necessarily run into the brook, was the means of communicating the disease to the thirteen persons taken ill on the night between the 9th and 10th of January." (p. 35.)

This surely cannot be considered as sound evidence. The mere *possibility* of the well being contaminated cannot be held sufficient in an inquiry demanding such accuracy and care in the collection of the facts.

8, 9. After an allusion to a very obscure Indian case, of which nothing can be made, Dr. Snow refers to the late attack of cholera in the Black Sea fleet. He quotes from the 'Medical Times and Gazette,' September 30th, the following passage from the letter of a naval medical officer:

"A week after the return of the fleet to Baljik, on the 7th of August, about four thousand French troops encamped on the heights abreast our anchorage. These were part of the first division of the army that had marched to Kostenje, about ten days before. By it the first blood had been drawn on the part of the allied army. The loss in battle was small, but they had encountered an enemy more terrible than the Russians. The cholera had broken out among them, and attacking four hundred on the first night, had destroyed sixty. The total loss had been something incredible. It was said, that out of eleven thousand men, not less than five thousand had perished in a few days. This dreadful calamity was attributed to drinking water from wells that had been poisoned by throwing in putrid carcases.

"Putting aside the question of intentional poisoning, which always presents itself as the most ready way of accounting for such destruction, perhaps some support to the theory, that water is the medium by which cholera poison is conveyed, may be found in this circumstance, and in another of which I was witness. These soldiers, wearied by marching from a focus of cholera infection, were seen many of them, washing their persons and clothing in the stream from which all the French ships of war, and the majority of the English fleet, obtained their supply of water. This was going on on the 7th and 8th, and, on the nights of the 9th and tenth, the disease burst out with great violence among the crews of several ships." (pp. 36, 37.)

We do not find this point alluded to in the 'Report' on this attack of cholera, just published by the Admiralty. It would appear, from that able document, that the main outbreak of cholera appeared in the fleet at Varna and at Baljik nearly at the same time. At the latter place, the intense outbreak was certainly from four to six days after the French troops had marched down; but it would appear that a solitary case had occurred on board the *Diamond* on the 16th of July, and that bowel complaints, gradually assuming a more severe form, and at last with "decided choleraic character," were common on board the fleet between the 1st and the 7th of August—i.e., the day that the French troops arrived to whom the outbreak is attributed—and on the 7th, a fatal case

occurred on board the *London*. The evidence, therefore, that connects these two circumstances—the arrival of French troops and the outbreak on board the ships—is not so stringent as it seems. But even if the French troops brought the cholera with them, the evidence of its being communicated by the water is most imperfect.

10. Dr. Snow makes no remark on the suggestion contained in the letter quoted in the 'Medical Times,' but proceeds to consider the terrible outbreak of cholera in a limited district near Golden-square, London, which occurred in 1854:

"There were a few cases of cholera in the neighbourhood of Broad-street, Golden-square, in the latter part of August; and the so-called outbreak, which commenced in the night between the 31st August and the 1st September, was, as in all similar instances, only a violent increase of the malady. As soon as I became acquainted with the situation and extent of this irruption of cholera, I suspected some contamination of the water of the much-frequented street-pump in Broad-street, near the end of Cambridge-street; but on examining the water, on the evening of the 3rd September, *I found so little impurity in it of an organic nature, that I hesitated to come to a conclusion.* Further inquiry, however, showed me that there was no other circumstance or agent common to the circumscribed locality in which this sudden increase of cholera occurred, and not extending beyond it, except the water of the above-mentioned pump. I found, moreover, that the water varied, during the next two days, in the amount of organic impurity, visible to the naked eye, on close inspection, in the form of small white, flocculent particles; and *I concluded that, at the commencement of the outbreak, it might possibly have been still more impure.* I requested permission, therefore, to take a list, at the General Register Office, of the deaths from cholera, registered during the week ending 2nd September, in the sub-districts of Golden-square, Berwick-street, and St. Ann's, Soho, which was kindly granted. Eighty-nine deaths from cholera were registered, during the week, in the three sub-districts. . . .

"On proceeding to the spot, I found that nearly all the deaths had taken place within a short distance of the pump. There were only ten deaths in houses situated decidedly nearer to another street-pump. . . .

"With regard to the deaths occurring in the locality belonging to the pump, there were sixty-one instances in which I was informed that the deceased persons used to drink the pump-water from Broad-street, either constantly or occasionally. In six instances I could get no information, owing to the death or departure of every one connected with the deceased individuals; and in six cases I was informed that the deceased persons did not drink the pump-water before their illness." (pp. 38—40.)

Dr. Snow then refers to workhouses and large establishments in the neighbourhood, which did not use the pump water, and the inmates of which escaped cholera. He then quotes the case of a gentleman who went to Poland-street, where his brother had just died of cholera, drank a small tumbler of water mixed with brandy, left the neighbourhood, and was seized with cholera on the following day. This, however, is inconclusive, as the person was in an infected neighbourhood.

Finally, Dr. Snow relates the following most extraordinary case, which, if there is not some fallacy, is certainly unanswerable:

"In the 'Weekly Return of Births and Deaths' of September 9th, the following death is recorded as occurring in the Hampstead district: 'At West-end, on 2nd September, the widow of a percussion-cap maker, aged 59 years, diarrhoea two hours, cholera epidemica sixteen hours.'

"I was informed by this lady's son that she had not been in the neighbourhood of Broad-street for many months. A cart went from Broad-street to West-end

every day, and it was the custom to take out a large bottle of the water from the pump in Broad-street, as she preferred it. The water was taken on Thursday, 31st August, and she drank of it in the evening, and also on Friday. She was seized with cholera on the evening of the latter day, and died on Saturday, as the above quotation from the register shows. A niece, who was on a visit to this lady, also drank of the water; she returned to her residence, in a high and healthy part of Islington, was attacked with cholera, and died also. There was no cholera at the time, either at West-end or in the neighbourhood where the niece died. Besides these two persons, only one servant partook of the water at Hampstead West-end, and she did not suffer, or, at least, not severely." (pp. 44-45.)

It will have been observed, that the contamination of the pump water with drains, or by any other method, is not ever attempted to be proved, and the disease had commenced to decline before the supply of the suspected water was stopped.

"The greatest number of attacks in any one day occurred on the 1st of September, immediately after the outbreak commenced. The following day the attacks fell from one hundred and forty-three to one hundred and sixteen, and the day afterwards to fifty-four. A glance at the table will show that the fresh attacks continued to become less numerous every day. On September the 8th—the day when the handle of the pump was removed—there were twelve attacks; on the 9th, eleven; on the 10th, five; on the 11th, five; on the 12th, only one; and after this time, there were never more than four attacks on one day. . . .

"There is no doubt that the mortality was much diminished, as I said before, by the flight of the population, which commenced soon after the outbreak; but the attacks had so far diminished before the use of the water was stopped, that it is impossible to decide whether the well still contained the cholera poison in an active state, or whether, from some cause, the water had become free from it. The pump-well has been opened, and I was informed by Mr. Farrell, the superintendent of the works, that there was no hole or crevice in the brickwork of the well, by which any impurity might enter; consequently in this respect the contamination of the water is not made out by the kind of physical evidence detailed in some of the instances previously related. I understand that the well is from twenty-eight to thirty feet in depth, and goes through the gravel to the surface of the clay beneath. The sewer, which passes within a few yards of the well, is twenty-two feet below the surface. The water at the time of the cholera contained impurities of an organic nature, in the form of minute whitish flocculi, visible on close inspection to the naked eye, as I before stated. Dr. Hassall, who was good enough to examine some of this water with the microscope, informed me that these particles had no organized structure, and that he thought they probably resulted from decomposition of other matter. . . .

"A very important point in respect to this pump-well is that the water passed with almost everybody as being perfectly pure, and it did in fact contain a less quantity of impurity than the water of some other pumps in the same parish, which had no share in the propagation of cholera. We must conclude from this outbreak that the quantity of morbid matter which is sufficient to produce cholera is inconceivably small." (pp. 51-54.)

Dr. Snow also observes, that he inquired of many persons if any change in the character of the water had been observed, and was answered in the negative. Mr. Gould, the eminent ornithologist, however, noticed that on the 2nd September "it had an offensive smell," although it was perfectly transparent.

The chief circumstances which Dr. Snow appears to rely upon are, that the great mortality took place in the district supplied with this pump-water, and that persons in the district who did not drink the

water escaped. He has not been able to prove that all were attacked who drank this water, and that none were attacked who did not drink; but such precision of evidence could not fairly be demanded from him.

The weak points in this array of evidence are, 1st, the want of proof of contamination of water, or, rather, the evidence in favour of its purity; 2ndly, the deficiency in negative evidence, that there was no other local cause which produced this partial outbreak; and, 3rdly, the fact that the disease ran rapidly to its acme, and then declined, while the water supply remained the same.

On examining a map given by Dr. Snow, it would clearly appear that the centre of the outbreak was a spot in Broad-street, close to which is the accused pump; and that cases were scattered all round this nearly in a circle, becoming less numerous as the exterior of the circle is approached. This certainly looks more like the effect of an atmospheric cause than any other; if it were owing to the water, why should not the cholera have prevailed equally everywhere where the water was drunk? Dr. Snow anticipates this by supposing that those nearest the pump made most use of it; but persons who lived at a greater distance, though they came farther for the water, would still take as much of it. We observe also that there are several other pumps in the neighbourhood, and in one of these the water was notoriously offensive; yet comparatively little cholera took place thereabout. There are, indeed, so many pumps in this district, that wherever the outbreak had taken place, it would most probably have had one pump or other in its vicinity.

11. The last case noticed by Dr. Snow is that of a limited outbreak in Deptford, which seems to us as deficient in weight of evidence as those which have preceded it.

"Deptford is supplied with very good water from the river Ravensbourne by the Kent Water Works, and until this outbreak there was but little cholera in the town. . . . On going to the spot on September 12th and making inquiry, I found that the houses in which the deaths had occurred were supplied by the Kent Water Works, and the inhabitants never used any other water. The people informed me, however, that for some few weeks the water had been extremely offensive when first turned on; they said it smelt like a cesspool, and frothed like soap suds. They were in the habit of throwing away a few pailsful of that which first came in, and collecting some for use after it became clear. On inquiring in the surrounding streets, to which this outbreak of cholera did not extend, viz., Wellington-street, Old King-street, and Hughes's-fields, I found that there had been no alteration in the water. I concluded, therefore, that a leakage had taken place into the pipes supplying the places where the outbreak occurred, during the intervals when the water was not turned on." (pp. 55—56.)

We have now given, as far as possible, in Dr. Snow's own words, an abstract of his evidence. In estimating its weight we must bear in mind the object for which it is adduced. It is not to prove that bad water acts as a predisposing cause, but that the water contains itself the cause of cholera. To prove so weighty a fact, we require not only positive, but negative evidence. If the cause of cholera cannot be absolutely discovered in the water, we must at least have proof that the water is contaminated, and we must also have negative evidence that no other circumstance existed which could explain the attack except the contaminated water. Now, certainly in no less than seven of the eleven cases

(Nos. 3, 5, 6, 7, 8, 9, and 11), the evidence to prove the effect of the water is so loosely stated, and the accessory circumstances of the outbreaks are so utterly disregarded, that we do not think any one can feel that even a tolerable case is made out in favour of Dr. Snow's opinion.

In the four remaining examples, the evidence is stronger. The case of the two courts at Horsleydown is the best, though it is not conclusive; and next to this is the instance at Salford quoted from the Board of Health, although even here the account is so meagre that we scarcely know whether to accept it. The outbreak at Albion-terrace, singularly localized as it was, is yet susceptible of another explanation, viz., that the air was contaminated by the bursting of the drain; and the attack in Broad-street wants entirely one material item of evidence, viz., proof that the water was contaminated; indeed, we have seen that Dr. Snow is here absolutely obliged to admit, that the water may be apparently pure, and that the quantity in it "of morbid matter sufficient to produce cholera is inconceivably small." If we accept this opinion, we can never exclude the agency of water in any case.

Considering, therefore, the imperfection of both the positive and negative evidence, and the want of explanation of the earliest cases, we conclude that Dr. Snow has not yet proved that cholera is *always* communicated by means of water; and that he has not even *proved* that it has been so communicated in a single case. Yet, in the face of the evidence, furnished by the attacks at Horsleydown and Salford, we cannot entirely reject Dr. Snow's views. We have made no reference to the other phenomena of the spread of cholera which Dr. Snow's hypothesis cannot explain; but have simply taken the facts given to us by Dr. Snow.

We must now, however, turn to the second part of Dr. Snow's book, in which a fresh argument for the influence of water is brought forward. Almost half of the work is taken up with a most elaborate inquiry into the water supply, as compared with the number of deaths from cholera. The object is to show, that when the water was supplied from an impure source, and was therefore probably contaminated with sewage matters, cholera was most prevalent.

It is impossible for us to go minutely into the mass of evidence so laboriously collected and arranged by Dr. Snow. In this part of his work, as in the former half, Dr. Snow does not sufficiently discuss the other conditions under which the people living in various districts of London were placed, besides those of varying water supply. He alludes, indeed, to, but speedily dismisses, the important law of the influence of elevation, demonstrated by the Registrar-General; and refuses, indeed, to admit the effect of elevation, and refers the difference of prevalence entirely to the water supply. He does not, in our opinion, pay sufficient attention to the density of the population, nor to the other causes of impurity of the air.

We shall now shortly advert to some part of the evidence.

In 1849, two water companies—the Southwark and Vauxhall, and the Chelsea—took their water from the same part of the Thames; the district supplied by the former company suffered severely, that supplied by the latter, very little. Dr. Snow explains this as follows:

"The Chelsea Company, which supplies some of the most fashionable parts of

London, took great pains to filter the water before its distribution, and in so doing no doubt separated, amongst other matters, the greater portion of that which causes cholera. On the other hand, although the Southwark and Vauxhall and the Lambeth Water Companies professed to filter the water, they supplied it in a most insipid condition." (p. 64.)

We doubt the correctness of this answer. Both companies filter, says Dr. Snow, but one filters more thoroughly than the other, and therefore gets rid of that which causes cholera. But what evidence is there, that at the time when cholera prevailed, the one company did filter so carefully, and the others so carelessly? There appears to have been no examination made of the water at the time, and certainly we are not inclined to believe, without definite testimony, that one company could purify their water completely, and the other not at all.

But in a later page we find Dr. Snow modifying his opinion, for he discovers a fact which shows that filtration is not all-powerful. Till lately, Milbank prison was supplied with Thames water, which was filtered most carefully through sand and charcoal, yet cholera prevailed in the prison. As filtration is not, then, sufficient, the purification of the Chelsea water, previously referred to filtration, is now ascribed to retention in the reservoirs causing the decomposition of the choleraic substance.

A second point of evidence on which Dr. Snow relies, requires rather a longer discussion.

Part of London, on the south side of the Thames, is supplied by the Southwark and Vauxhall Company, and part by the Lambeth Company; the former takes its water from Battersea, the latter from Thames Ditton. One portion of the town, however, is supplied by both companies; some houses being supplied by one, some by the other company. Now, it is evident, that if in this part of London it should be found that the inmates of the houses supplied with the Battersea water suffered greatly, and those supplied with Ditton water suffered little, then of course, as all other circumstances (locality, air, trades, &c.) in the district are the same, the influence of the water would be proved beyond a doubt. Dr. Snow, with most praiseworthy industry, proceeded to investigate the point:

"The inquiry was necessarily attended with a good deal of trouble. There were very few instances in which I could at once get the information I required. Even when the water-rates are paid by the residents, they can seldom remember the name of the water company till they have looked for the receipt. In the case of working people who pay weekly rents, the rates are invariably paid by the landlord or his agent, who often lives at a distance, and the residents know nothing about the matter. It would, indeed, have been almost impossible for me to complete the inquiry, if I had not found that I could distinguish the water of the two companies with perfect certainty by a chemical test. The test I employed was founded on the great difference in the quantity of chloride of sodium contained in the two kinds of water at the time I made the inquiry. On adding solution of nitrate of silver to a gallon of the water of the Lambeth Company, obtained at Thames Ditton, beyond the reach of the sewage of London, only 2.28 grains of chloride of silver were obtained, indicating the presence of .95 grains of chloride of sodium in the water. On treating the water of the Southwark and Vauxhall Company in the same manner, 91 grains of chloride of silver were obtained, showing the presence of 37.9 grains of common salt per gallon. Indeed, the difference in appearance on adding nitrate of silver to the two kinds of water was so great, that they could be at once distinguished without any further trouble.

Therefore, when the resident could not give clear and conclusive evidence about the water company, I obtained some of the water in a small phial, and wrote the address on the cover, when I could examine it after coming home. The mere appearance of the water generally afforded a very good indication of its source, especially if it was observed as it came in, before it had entered the water-butt or cistern; and the time of its coming in also afforded some evidence of the kind of water, after I had ascertained the hours when the turacocks of both companies visited any street. These points were, however, not relied on, except as corroborating more decisive proof, such as the chemical test, of the company's receipt for the rates." (p. 77, 78.)

We looked now anxiously for the result of this inquiry, and found it stated further on :

"There were three hundred and thirty-four deaths from cholera in these four weeks, in the districts to which the water supply of the Southwark and Vauxhall and the Lambeth Company extends. Of these it was ascertained, that in two hundred and eighty-six cases the house where the fatal attack of cholera took place was supplied with water by the Southwark and Vauxhall Company, and in only fourteen cases was the house supplied with the Lambeth Company's water; in twenty-two cases the water was obtained by dipping a pail directly into the Thames; in four instances it was obtained from pump-wells; in four instances from ditches; and in four cases the source of supply was not ascertained, owing to the person being taken ill whilst travelling, or from some similar cause." (pp. 79, 80.)

On first reading this paragraph, we thought that the deaths referred to, took place only in the district with the intermingled supply, and that this was the answer to the "experiment on a grand scale," so laboriously inquired into by Dr. Snow. But, on re-perusing the passage and its context, we found that these deaths had taken place in *all* the districts supplied by the two companies, separately or conjointly. If this reading be correct, we doubt if the comparison can be safely made, for the Lambeth Company supplies, to a considerable extent, a good neighbourhood on elevated ground (including the healthy districts of Streatham, Forest-hill, and Sydenham); while the Southwark and Vauxhall Company supplies the greater part of the poorest, lowest, and marshiest district in London.

If, however, the deaths referred to in the paragraph just quoted, represent those only of the subdistrict supplied conjointly by the two companies, we have still some grounds of objection. First, are we to rely on the chemical test referred to by Dr. Snow? Does the Ditton water never contain *more*, and the Battersea *less*, chloride of sodium? Dr. Snow himself mentions that :

"When the water (Battersea) of the Southwark and Vauxhall Company was examined by Messrs. Graham, Miller, and Hofmann, at the latter part of January, 1851, it contained only 1.99 grains of chloride of sodium, or about one-twentieth as much as it contained last September, and one fifteenth as much as on 21st November, 1854." (p. 97.)

May not this small amount have occurred on some of the days on which water was collected by Dr. Snow? Did he ascertain that the water in the reservoirs of the company contained always this large amount of chloride of sodium during the prevalence of cholera, or was the water he tested merely taken from the cisterns supplied by the Southwark and Vauxhall Company? We certainly do feel great doubt, on the evidence

before us, whether the source of supply could safely be inferred from a chemical test alone.

Another weak point in the argument seems to us to vitiate Dr. Snow's whole case. He has not told us how many houses in the district referred to were supplied respectively by the Battersea and Ditton waters. It may be that the Ditton water was furnished to so few houses, that the small number of deaths from cholera were merely proportionate to the smaller number of houses. Dr. Snow endeavours to meet this difficulty, by giving, from the Parliamentary return, the number of houses supplied respectively by the two companies. But this return applies to the entire districts, and not to the special district where the supplies are intermingled; so that really we are in doubt whether the Ditton water is supplied to half of this special district, or to a quarter or a tenth part of it.

However, we learn from the Parliamentary return that the total number of houses supplied by the Southwark and Vauxhall Company is 40,046, and by the Lambeth Company 26,107, or as three to two, nearly. If this proportion be the same in the district common to both (of which, however, we have no certainty), then the deaths, of course, should properly be one-third more numerous in the houses supplied by the Southwark and Vauxhall than in those of the Lambeth Company. The difference in the mortality is, however, much more than this, if Dr. Snow's facts are to be received.

A better instance of the influence of water than either of these cases is given by the Lambeth Company. In 1849, it drew its supplies from Battersea; in 1854, from Thames Ditton. Was the mortality in the districts supplied by it diminished in the latter year? Of this there is proof, so far, that whereas the districts supplied solely by the Southwark and Vauxhall, show an equal mortality in the two years, those supplied partly by the Lambeth, as well as the Southwark and Vauxhall Company, show in several cases a remarkable diminution of mortality.

We shall not follow Dr. Snow into his account of the provincial towns, where we find little that is satisfactory. Here, as in all other cases, Dr. Snow refers only to the water-supply, and neglects all the other circumstances, as we could easily prove, had we space to refer to the cases of Birmingham, Leicester, or Exeter. The conclusions as to the attack at Newcastle do not appear to us to be borne out by the evidence.

Besides cholera, Dr. Snow thinks other diseases, yellow fever, intermittents, plague, and typhoid fever, may be also propagated by drinking water. The evidence for this is so slight, that we shall not discuss the point now.

* We have already said, that from the positive evidence adduced by Dr. Snow, we were unable to do more than conclude that he had rendered the transmission of cholera by water an hypothesis worthy of inquiry; we cannot draw any other conclusion from his researches on water supply, than that the predisposing effects of impurity of water are also rendered highly probable. We may be mistaken in this, and the evidence which seems weak to us may not be so to others. If so, when additional evidence shall be given, we shall receive it with the greatest pleasure; for though we think

Dr. Snow's hypothesis, if proved, cannot explain all the phenomena of the spread of cholera,* it would yet clear up some of the mysterious phenomena of its diffusion. Its establishment would therefore be an immense gain to science, and, we need not add, an important service to the State.

We cannot conclude without one remark. We have taken Dr. Snow's facts, and have not only criticised them as carefully as we could, but have expressed our opinions without reserve. In no other way could we treat this all-important question; and Dr. Snow, we are sure, would not have wished us to adopt any other course. But it is only simple justice to Dr. Snow to state, that no man could have pursued the inquiry with greater diligence. Although we think that he is biased by his creed,† and obstinately looks only in one direction, we close his book with the conviction that he is an honest and conscientious observer. If his discovery should be established, the prevention of cholera would be easy, and for this reason we think that the most careful inquiry should be made at once by the government into the subject, and proof or disproof given of Dr. Snow's opinions. In India, the point could be soon decided.

E. A. Parkes.

REVIEW XIV.

Vertigo: a Paper read to the North London Medical Society, April 12th, 1854. By J. RUSSELL REYNOLDS, M.D. Lond., University Medical Scholar.—London, 1854. 8vo, pp. 46.

WE are glad that Dr. Reynolds should have taken an opportunity of directing the attention of his professional brethren to the subject of vertigo, because we believe that the time is come at which a far more precise estimate can be formed respecting the nature of this condition, than was possible whilst the physiology of the sensori-motor apparatus was in a less advanced state; and because a careful appreciation of its phenomena will not only increase its semeiological value, but will, in its

* We would mention also here the experiments of Dr. Lander Lindsay, who appears to have given dogs cholera by making them breathe a choleraic atmosphere, but who, like others before him, could not cause cholera by feeding the dogs on cholera dejections.

† As an instance of this we may cite Dr. Snow's mode of accounting for the outbreak of cholera in London in 1854. On the 25th of July, the mate of a merchant steamer which had returned three weeks previously from the Baltic, died of cholera in London. "This patient was the chief-mate to a steam-vessel taking stores to, and bringing home invalids from, the Baltic fleet. Three weeks ago he brought home in his cabin the soiled linen of an officer who had been ill. The linen was washed and returned." The time when this steam-vessel arrived in the Thames with the soiled linen on board, was a few days before the first cases of cholera appeared in London, and these first cases were chiefly amongst persons connected with the shipping in the river. It is not improbable, therefore, that a few simple precautions, with respect to the communications with the Baltic fleet, might have saved London from the cholera this year, or, at all events, greatly retarded its appearance." Was any deduction of so extraordinary a kind ever made on such grounds? We are not told that the officer whose clothes were brought home had had cholera; we are not told where the clothes were washed, by whom they were washed, or when they were washed. Supposing the mate to have sent them to be washed the day he arrived—three weeks before his death—as is most probable, can the most perverse ingenuity connect his death with these clothes? And then with regard to London itself, as many cases of cholera had occurred before July, how is it possible for one moment to adhere to the hypothesis that the soiled linen of a sick officer in the Baltic fleet being washed in the Thames was the cause of the epidemic?

turn, help to elucidate the normal actions of that part of our organization. Our author uses the term *vertigo* in a more extended sense than that in which it is usually understood: making it to comprehend not merely vertiginous *sensations*, but also vertiginous *movements*. And he proceeds first to the examination of the latter class of phenomena, because he thinks that they throw some light on the nature of the former.

A very complete historical summary is given of the phenomena of *vertiginous motion*, as they present themselves to the observer of natural disease, or to the physiological experimenter; and from the latter order of facts the conclusion is drawn, that the known occasions of vertiginous movement are lesions of one or more of the following parts:

"1. The transverse or middle crura of the cerebellum, with their continuation upon the Pons Varolii.

"2. The crura cerebri.

"3. The thalami optici.

"4. The medulla oblongata.

"5. The tubercula quadrigemina.

"6. The semicircular canals of the ear.

"7. The eye itself." (p. 15.)

Now, as the functions of these organs, so far as we are able to assign them with probability, are as follows—

1. The centre of co-ordination;

2. The centres of common sensation and mobility;

3. The organs of the special senses (particularly of sight and hearing);

4. The fibres placing these organs in functional relationship;

the following general conclusion may be drawn:

"The vertiginous motions follow injuries to the organs of special sense, the organ for the co-ordination of movement, and the fibres connecting the latter with the former, or with the systematic muscles. In either case, some part of the nervous system, or its appendages, ministering to the '*consensus* of the nerves,' is injured, and the mysterious conversion of sensorial impressions into motor impulses is disturbed, or prevented from taking place." (p. 22.)

We believe, with Dr. Reynolds, that the real clue to the nature of this condition is to be found in that relation of muscular movement to *sensational* states, which has gradually come to be more distinctly marked-out by physiologists, in consequence of the exclusion of the excito-motor actions (*not* involving sensation) on the one hand, and of the emotional and volitional movements on the other. And this clue has been thus sagaciously followed up by our author. After pointing out our dependence upon guiding sensation in the execution even of our most purely volitional movements, he continues:

"The lower animals probably depend much more extensively upon the indications of sense, than do those possessed of greater intelligence, spontaneity, and educability; and, *à fortiori*, much more closely than Man. His energy of volition is displayed in subjecting thought, emotion, and muscular action, to the dictates of judgment; and it is certain that the power of doing so varies widely in individuals. In accomplishing the subjugation of the latter (muscular action), and in its direction to definite ends, he is guided mainly by sensation, and principally by touch, sight, and the muscular sense; and all our involuntary attitudes and motor impulses to change an unpleasant position are directed in the same manner. There is a feeling of physical rest or equilibrium, which we strive (it may be involun-

tarily) to attain by certain movements, and in this effort we are guided by impressions from without, and by the sense of our own muscular conditions. This feeling of equilibrium results from the harmony of our different sensations among themselves, and with the motor impulse which is their combined effect. When, therefore, any one group of the sensorial impressions is distorted or removed, the balance is *pro tanto* disturbed; and inasmuch as these impressions are themselves the stimuli of muscular action, attempts are made for its restoration. In the lower animals these attempts become much more marked than in Man, producing vertiginous or allied movements, which he, by a judgmatic discrimination between the inharmonious impressions, and by a volitional corrective power (which they do not possess to the same degree), is able to avert. The vertiginous movements, then, are the result of an effort to produce equilibrium; an effort developing itself in muscular action, through the agency of the nervous system, and under scusalional guidance." (pp. 24, 25.)

Dr. Reynolds next considers *vertiginous sensations*, which he defines to be "the sensation of motion without (or independently of) its real existence." This sensation, he remarks, may be either *objective* or *subjective*; the motion being referred, in the former case, entirely or principally to surrounding objects; whereas, in the latter, it is referred to the person of the individual himself. We do not think these terms happily selected, because they more properly apply to the distinction next drawn by Dr. Reynolds, between the vertigo originating in some cause external to the individual, and therefore referable to some change in his relationship to surrounding objects; and that which is not dependent upon any such change, its immediate cause being internal. It is only when employed in this latter sense that the terms have a meaning parallel to that which attaches to them when they are used to designate sensations of other kinds; and in this very mode they are employed by Dr. Reynolds himself in a later part of his essay.

Most persons are personally familiar with the vertigo which originates in impressions on the organs of sense, especially those of sight; and an analysis of their own feelings will satisfy them that, when not produced by the movement of surrounding objects or by movement of the individual himself, they result from his finding himself placed, to use Dr. Reynolds's phrase, "in some totally unaccustomed relationship with the world around him." An instance of this kind lately occurred to the writer of this notice. He had never experienced the least giddiness in looking down from the top of a high building or a lofty precipice; but on lately mounting to the prospective gallery at the summit of one of the slender minarets of the Panopticon, in Leicester-square, and finding himself sustained, as it were, in mid-air without that feeling of firm support which is given by the proximity of a *mass*, he felt an incipient vertigo which warned him to descend in time; and he has learned that the experience of several others has corresponded in this respect with his own. The same state may be produced by various *internal* causes; and that which most clearly connects the two classes of phenomena is the production of vertiginous sensations by the *remembrance* of past feelings, as when some "sensitive subjects" are made giddy by the sight of another person ascending a ladder or looking down from a precipice, through being led by association to recur to their own former experience in like circumstances. The state of the nervous centres, on which vertiginous sensations immediately depend,

may, however, be induced by various physical conditions; and these Dr. Reynolds distinguishes, according as they affect the nervous system primarily or secondarily, into—1. "Conditions of the nervous system," as changes in the circulation, organic lesions, or toxic influences; and, 2. "Conditions of the general system," as pyrexia, cachexia, toxæmia, or particular organic disturbances (of the stomach, for example). We cannot, however, see the justice of this classification; for, in all cases, a disordered action of the nervous system must be the immediate antecedent, or proximate cause, of the vertiginous sensations; and the changes in the circulation, which constitute part of his first class, surely belong to the same category with the pyrexia of the second, and the toxic influences of his first class to the toxæmia of the second. The only natural arrangement seems to us to be that which should group the causes according as they affect—1. The *organic structure* of the nervous centres, either by mechanical injury or by perversion of their nutrition; and, 2. Their *functional power*, by alterations in the supply or quality of blood, or by nervous sympathy with remote organs.

"It must be remembered," Dr. Reynolds justly remarks, "that, with the whole of our extended organism, the nervous centres are placed in intimate relation, receiving from its several parts indications or impressions which become the impulses to varied acts and functions; and that the balance of conscious health depends upon a thousand unconscious stimuli. The effects which similar static or dynamic diseases of different organs induce through the nervous centres vary widely; there is sickening depression from one, irritability from another, and dull hypochondriac from a third; but from almost any disturbance there may be vertigo, although it is, perhaps, more frequently connected with derangement of stomach and liver than of any other vegetative organ." (p. 43.)

We have ourselves witnessed a curious case, in which not only vertiginous sensations, but vertiginous movements (a convulsive turning to the right), were produced by the presence of indigestible food in the stomach; the symptoms, which at first seemed very alarming, passing off completely when the stomach had emptied itself by vomiting.

In concluding the short 'Memoir' of which we have thus noticed the more prominent points, Dr. Reynolds remarks upon the importance of more accurately noting the phenomena of vertigo than has been usually done; its mere occurrence, as one of the symptoms of a disease, being usually alone recorded, and no notice being taken of the varied modes in which it manifests itself, or of the different bodily conditions with which it seems connected. In order to direct attention to these, he has drawn up a useful scheme in which the points of chief interest are systematically arranged; and expresses the anticipation, in which we believe him to be fully justified, that—"If the diagnostic worth of this symptom can be increased, not only will an addition be made to the science of pathology, but we shall receive some aid in the more slowly-progressing science and art of therapeutics." And it has especially been rather with the view of opening a path for future investigation than of presenting any practical results already gained, that he has brought the subject before the profession.

PART SECOND.

Bibliographical Records.

ART. I.—*Medical Jurisprudence*. By ALFRED S. TAYLOR, M.D., F.R.S., &c. &c.—London, 1854. Small 8vo, pp. 935.

IN the year 1852, the fourth edition of this most comprehensive and useful Manual extended to eight hundred and twenty-six closely-printed pages. That edition speedily disappeared from the publisher's shelves, and two years afterwards the fifth (the present) edition was printed, and extended to one hundred and ten more pages. Including this edition, there have issued from the press *ten thousand seven hundred and fifty copies* of this work in the short period of eleven years! Much more need not be said about it; certainly in this instance commendation would be supererogation. We may, however, indicate the parts which have received the greatest amount of additions. These are chronic poisoning; the operation of prussic acid, morphia, strychnia, and aconite; wounds; injuries; blood-stains; burning of the human body, and spontaneous combustion; infanticide; pregnancy; abortion; gestation; insanity. This new matter comprises reports of most of the important trials that have involved medico-legal evidence. These additions are diffused throughout the work, and illustrate each part according to the relative cases or observations.

Obviously, we can, with such an abundance of material, do no more than record our conviction, derived from a close perusal of every page of the work, that Dr. Taylor has brought this edition up to the very level of medico-legal science, and maintains therein the reputation he has achieved, and which has not only placed him at the head of British legal medicine, but has redeemed the same from the imputation under which it long laboured, of having no literary representation.

ART. II.—*Handbuch der Speciellen Pathologie und Therapie*. Redigirt von R. VIRCHOW, Professor in Würzburg.
Manual of Special Pathology and Therapeia.

THIS great work, edited by Virchow, and written by himself and by many of the leading physicians of Northern Germany, is rapidly approaching completion. The first volume is finished, and parts of the second, fifth, and sixth volumes have been published. As the character of the work unfolds itself, however, we are sorry to find it assuming too

much the character of an encyclopædia. The immense length and fulness of detail with which many of the subjects are treated, appear to us quite to destroy its simple character of 'Handbook.' The diseases of the lungs are prefaced by a most elaborate, but most misplaced, treatise on the acoustic phenomena of auscultation and percussion, which is practically of little use. No less than three hundred and thirty-one closely-printed pages are filled by Dr. Falck, with an account of the various "Intoxicative agents." Yet this part, though quite too long in proportion to other subjects, is so admirably done, that we should regret to see it shortened. The diseases of the male genital organs and bladder (without ~~syphilis~~ *syphilis*) are described by Pitha at enormous length, two hundred and eleven pages being given to this subject alone. At this length, we tremble to think how much space, diseases of the heart, lungs, or brain, must occupy.

The first part of the diseases of the chylopoietic viscera, by Bamburger, occupies three hundred and seventy-six pages, and as much more will be required to complete the subject. Lebert discusses, in one hundred and fifty-two pages, the diseases of the blood and lymph vessels. Simon, the well-known dermatologist, describes syphilis at great length; and Virchow narrates, with his accustomed power, the account of the diseases communicated from animals to man.

ART. III.—*Food and its Adulterations; comprising the Reports of the Analytical Sanitary Commission of the Lancet.* By ARTHUR HILL HASSALL, M.D.—London, 1855. pp. 659.

WE are not aware that so complete a work on the adulteration of food has ever been published in any language as the one now before us. The labour which has been given to its production must have been excessive, and we are astonished that any one man could have found time enough to prosecute so vast an inquiry with such success. To analyze such a work is impossible, and we must satisfy ourselves with directing the attention of our readers to it, and with assuring them that the information contained in it should be familiar to every medical practitioner.

When these reports originally appeared in the 'Lancet,' the names of the dealers from whom the various substances were obtained, were published, in order, apparently, to check as much as possible the practice of adulteration, by showing dishonest tradesmen that the law would not protect them from deserved exposure. It is remarkable that no action has been brought against the 'Lancet,' for any of the statements made by Dr. Hassall in its columns. This is itself a guarantee of the correctness of the facts given by the author.

The adulteration of food is one of the most important subjects which can be considered by any government; and various have been the proposed schemes for preventing or limiting this most hurtful practice. The author proposes that a central Board, with analytical chemists attached to it, whose duty it should be to look out and detect adulteration in wholesale and retail practice, and to bring the offenders under the penalties of the law, should be instituted. Some such plan as this is, we believe, indis-

pensable. But as the knowledge of the microscope and of chemistry is now so general, it would be an accessory means of great importance to publish, in some cheap and accessible form, accurate microscopical representations of pure articles of food, and of the substances with which they may be adulterated; and to give brief directions for the application of chemical tests, when these are to be used.

A work of this kind, if published cheaply by authority, would soon find its way everywhere; those who have not already learnt to use the microscope, would acquire the art; and as every tradesman would know that many of his customers would have an infallible means of detecting imposition, food would very soon cease to be adulterated. The diffusion of practical knowledge of this kind would be better than a host of inspectors. We should advise Dr. Hassall to write a short work of this kind, in which his illustrations might be used. It would be one of the most useful works of the time.

ART. IV.—*Report on the Mortality and Public Health of Oxford, during the years 1849–50.—Oxford.* (Printed for the Ashmolean Society.) 1854.

WE owe this very valuable addition to statistical and sanitary knowledge to the labours of Dr. Greenhill. The Report is not carried down later than 1850, on account of the removal of the author to Hastings; but we trust that some one will have labour and zeal enough to continue the work so well commenced. The average annual mortality of Oxford was, during these two years, 24.09 to every one thousand persons living; and of the deaths no less than 24.73 per cent. were caused by zymotic diseases. The fatality was greatest in autumn and winter; summer was healthier, and spring healthiest.

In an appendix, an interesting account of the cholera in 1849 is given. The epidemic was much lighter than that of 1832; the introduction of the disease could not be traced to personal intercourse, nor was there any strong evidence of contagion observed during its course; yet Dr. Greenhill informs us, that at the close of the epidemic "it was the general conviction that cholera was occasionally communicated directly or indirectly by personal intercourse." One part of the town, which had suffered severely in 1832, was scarcely affected in 1849, and this is ascribed to improvement in the water supply.

ART. V.—*Notes on some of the Developmental and Functional Relations of certain Portions of the Cranium.* Selected by F. W. PAVY, M.D. Lond., from the Lectures on Anatomy delivered at Guy's Hospital by JOHN HILTON, F.R.S.—London, 1855. pp. 93.

THESE 'Notes' are by no means dry details of the anatomy of the cranial bones, but are interesting disquisitions on various surgical and physiological problems, which naturally arise for discussion when the bones of the head are being demonstrated.

As an example of this we may quote one or two passages:

"On examining the interior of the adult cranium by transmitted light, certain ridges or elevations of bone are observed connected with the internal osseous plate; which, although pursuing an irregular and tortuous course, yet may be distinctly traced to converge towards the anterior clinoid processes of the sphenoid and the petrous portions of the temporal bones. In accordance with this anatomical arrangement, and with the physical laws of nature, vibrations derived from a slight blow, or other external influence; instead of being diffused over the walls of the skull, meeting on the opposite side, and producing the injurious effects of *contre-coup*; travel along these ridges, which, by virtue of their greater thickness and solidity, form better conductors of vibrations than the surrounding parts, and converge towards the anterior clinoid processes, and the petrous portions of the temporals, where they terminate in the following manner.

"The anterior clinoid processes lie surrounded by cerebro-spinal fluid, without having, as I have already said, the slightest degree of connexion with the superimposed brain. And the vibrations conducted to these points, become here broken or lost in this fluid, which thereby intercepts their transmission to the tissue of the cerebral organ.

"The petrous portions of the temporal bones, being separated from the basilar process by an intervening layer of soft or membranous structure, the vibrations conducted in this direction become here also completely obstructed without transmission to the brain. Were it not, indeed, for this interception to the transmission of vibrations, by the interruption to the osseous continuity between the petrous portions of the temporal bones and the basilar process of the sphenoid and occipital, the latter would be constantly exposed to serious concussions, or even the chances of fracture, at the angle or point of collision of the vibrations travelling along and meeting from each petrous portion of the temporal." (pp. 42, 43.)

Afterwards, the same fact is noticed in another way:

"I have previously, by means of preparations copied from Nature with the greatest care and accuracy, shown you that the various elevations and depressions observed on the internal aspect of the dome of the skull do not correspond with the opposed surface of the brain. It is therefore evident, as I have also already stated, that they cannot be intended for adaptation to the concussions of the cerebral hemispheres, and, if we examine them with care and attention, we find that they present a definite and designable arrangement: the elevated portions forming undulatory ridges which converge towards certain points projecting from the base of the skull. These ridges, or prominences, no doubt, give greater strength and additional security to the osseous parietics; but, forming the most dense and solid parts of bone, they also act as better conductors of vibrations; and, therefore, as it were, determine the course of their transmission from their seat of origin, in a blow, or otherwise, on the exterior of the head." (p. 52.)

The observations on the effects of the growth of the sphenoid are extremely interesting:

"The primary idea or primary intention of the development of the sphenoid seems chiefly with reference to the masticatory function; but, in the changes that it produces in the direction of the cranial and facial bones, it may not unaptly be compared to the scaphoid bones of the carpus and tarsus; for, in its growth and final development, it effects for the cranium and face precisely the same object that these bones effect for the hand and foot. . . .

"Like these bones, the growth and completion of the sphenoid, in spreading out the cranium, and in enlarging the cavities of the organs belonging to the face, supplies the deficiency of the muscular tension, which in other parts of the body has so large a share in determining the final or perfect form of the bones. For example, with regard to the long bones of the extremities, the surrounding muscular tension, acting in different directions, forms a material influence in the determination of their precise ultimate configuration. In the case of the cranium, no

such comparable muscular tension exists. Indeed, did it exist, it would rather tend to compress than to expand the cranial cavity. But its absence is supplied by the growth and development of the sphenoid, which, wedged in amongst the other bones, alters their position and direction, and thus influences the ultimate configuration of the whole cranium and face." (pp. 73, 74.)

Mr. Hilton then points out the change in the direction of the petrous bones which occur during growth, and traces this to the expanding effect of the spinous processes of the sphenoid. The effects, also, on the temporal, and, through them, on the parietal bones, are then clearly narrated. These and other points are illustrated by some very admirable plates.

We are sorry to find, from the preface, that the *Guy's Hospital Reports* (in which part of this volume has appeared) are definitively given up. We regret this extremely, as there is not merely much historical interest connected with this journal, but the later numbers have contained very admirable papers, and are not unworthy to be placed by the side of those which contain the classic articles of Bright or Addison. We trust, however, to see the *Guy's Hospital Reports* some day resuscitated.

ART. VI.—*The Prize Essay on the Changes since the time of Pinel in the Moral Management of the Insane, &c. &c.* By DANIEL H. TUKE, M.D., Assistant Medical Officer to the York Retreat.

THE Society for Improving the Condition of the Insane will assuredly have done much towards the accomplishment of their benevolent object, by the publication of Dr. Tuke's essay. The very name of Tuke, so honourably associated with the York Retreat (the author is the great-grandson of the founder), is itself of good omen. In this slight volume of 119 pages are presented with an admirably-written history of the gradual revolution which has been effected in the treatment of the insane in public institutions,—a change from barbarism, and its accompanying ignorance and ferocity, to the enlightened mildness of growing civilization: and, nevertheless, a change that can scarcely be dated before the present century. The names ever to be connected with this reformation are Pinel, Tuke, Charlesworth, Hill, and Conolly. The last honourable name deserves particular commemoration, because the bearer of it had the courage to try the experiment of non-restraint on a very large scale, and under the inspection of jealous witnesses; carrying it out with a noble mixture of firmness, patience, and forbearance, in spite of censure and ridicule; and commending it to the reason and the humanity of the public and the profession, in a quick succession of reports, essays, lectures, and reviews, worthy of a pen that has so often done good service in medical literature. We have already praised the execution of Dr. Tuke's essay, and we now only add, that it is not only clear and succinct as a history, but that it is also calm, unimpassioned, and free from any attempts at rhetorical denunciations of the old system, or from enthusiastic encomiums on the new. The reader will find many valuable hints as to methods of treatment to be used in substitution of mechanical restraint.

ART. VII.—*The Pathology of Drunkenness.* By CHARLES WILSON, M D.
—*Edinburgh, 1855.* Small 8vo, pp. 230.

THE object of the writer has been to present to the general, as well as to the scientific reader, a sketch of the effects of drunkenness on the mind and body. He has produced an extremely instructive and readable book, in which the results of very extensive reading and observation are embodied. We most strongly recommend the work to all our Temperance Societies, as it would be a worthy companion to the treatises on the use and abuse of alcohol, by Drs. Carpenter and Spencer Thomson, which have already so much assisted the cause of Temperance by diffusing a correct knowledge of the action of alcohol on the frame.

We shall not fail to analyze Dr. Wilson's book at length, on some future occasion.

ART. VIII.—*Transactions of the Belfast Clinical and Pathological Society for the Session 1853-4.*—*Belfast, 1854.*

THIS is an admirable little volume, and contains many well-reported and interesting cases, and many valuable remarks and discussions on them.

In addition to the exhibition of morbid specimens, the Belfast Society have adopted the practice of proposing certain queries, to be made the subject of debate.

We shall probably make further use of this volume, and shall take the opportunity of comparing it with the 'Transactions' of the London Society.

ART. IX.—*Archives de Physiologie, de Thérapeutique, et d'Hygiène.*
Par M. BOUGHARDAT. No. II. Octobre, 1854. *Mémoire sur l'Action Physiologique et Thérapeutique des Ferrugineux.* Par T. A. QUEVENNE.—1854. pp. 356.

On the Physiologic and Therapeutic Action of Preparations of Iron.

THE first number of this serial was entirely occupied with the researches of MM. Homolle and Quevenne on "Digitaline, and its Action." The second number contains a not less elaborate inquiry by the last-named gentleman into the action of the ferruginous preparations. "Iron," says M. Quevenne, "is an essential constituent of the body; introduced into the stomach, its effect is to render the alimentary substances more easily precipitable, by contact with the serum of the blood, and more readily transformed into globules. Introduced into the circulation, it is a carrier of oxygen. In the ordinary conditions of life, sufficient iron is introduced with the food; when iron is deficient, it must be added to the food. Of all the insoluble preparations, the iron reduced to the metallic state by hydrogen (*fer réduit*) is most easily assimilated." These propositions contain nothing novel, but they are illustrated and placed on a secure basis by a vast number of carefully conducted experiments, which render this work a very valuable contribution to therapeutical knowledge.

ART. X.—*Lithotomy Simplified*. By GEORGE ALLARTON, M.R.C.S.
London, 1854. pp. 80.

THE mode of operating practised by Mr. Allarton is a modification of an old process, and is thus described :

"I introduce a grooved staff in the usual manner, and of the usual size, and confide it to an assistant, with directions to keep it perpendicular and hooked up against the pubes; I then introduce the index finger of my left hand into the rectum, placing its extremity in contact with the staff, as it occupies the prostate, and press it firmly against the staff, so as to steady it; then, with a sharp-pointed straight knife, with tolerably long and rough handle, I pierce the perineum in the middle line, about half an inch above the anus, or at such distance as may appear necessary to avoid dividing the fibres of the external sphincter,—I carry the knife steadily and firmly on till it strikes the groove of the staff, the deep sphincter lying between the knife and the directing finger, which enables me to judge of the distance as the knife passes along. If the incision be not made exactly in the median line, the contracting fibres of the injured muscles draw the point of the knife from its direct line, and interfere with the accuracy of striking the staff. Having struck the groove of the staff, I move the point of the knife along the groove towards the bladder a few lines, and then withdraw it, cutting upwards, so as to leave an external incision of from three quarters of an inch to one and a half inches, according to the presumed size of the stone—the escape of urine indicates the entrance to the urethra. I then introduce a long ball-pointed probe or wire through the external opening into the groove of the staff, and slide it into the bladder, to sufficient depth to insure its safe lodgment in that viscus, and withdraw the staff. I then well grease the index finger of the left hand and pass it along the probe, with a semi-rotary motion, through the prostate into the bladder; which procedure is achieved without difficulty, and when the stone is free it comes at once into contact with the finger, and, if of moderate size, passes at once into the wound on withdrawing the finger, the patient having power to strain upon and thereby facilitate the extraction of the stone; this last-mentioned power being one of the great advantages of this operation. The incision being made strictly in the median line, no muscles are divided, and the integrity of the bladder being preserved, it is under the control of the patient, who exerts, at the wish of the surgeon, a powerful propulsive effort which keeps the stone in or in contact with the internal extremity of the wound, where it is easily seized by the forceps and extracted by mild persevering traction. Now, as the aperture is necessarily the size of the finger which produces it, if the stone be large some other dilating power must be employed in addition to the dilating effect of the forceps and stone combined; for this purpose Weiss' three-bladed female dilator, Arnott's hydraulic dilator, or, what is at once ready and effective, the addition of the vulcanized India-rubber finger-stalls one over another until the finger is sufficiently enlarged for the purpose, the outer covering being well lubricated with lard before being introduced. But Arnott's dilator, where it can be procured, is by far the most efficacious though not the most expeditious means." (pp. 22—26.)

The advantages of this operation are said to be—

"The impossibility of missing the bladder—the smaller amount of cutting than in the lateral operation—the neck of the bladder being uninjured—the smaller amount of blood lost—the prostate being merely dilated, not incised—the urine being at once passed by the urethra as well as by the wound, unless union by the first intention be effected—the facility with which the stone is reached, the patient being able to propel it towards the wound—the very short distance between the external opening and the interior of the bladder—the capability of breaking or crushing the stone, and washing out the bladder and freeing it from any minute particles—the small amount of pain—the absence of danger from urinary infiltration

—no muscle or vessel of any consequence being divided, no subsequent imperfection can arise—no danger of wounding the rectum—the rapid recovery, the patient being able to go about the next day—and the great facility with which the operation can be done by any practitioner of ordinary skill and ability.” (p. 84.)

Without using any dilator, Mr. Allarton has extracted a stone one inch and a quarter in diameter, and he believes a stone of two inches diameter might be extracted with Arnott's dilator. For any larger mass he would suggest, it appears, crushing the stone, after the bladder has been opened.

We should have been glad to have been informed how many cases have been operated upon, how long each operation lasted, and how often the dilator has been used.

ART. XI.—*The Practitioner's Pharmacopœia.* By JOHN FOOTE,
M.R.C.S. Lond.

A LITTLE work, called ‘The Prescriber's Pharmacopœia,’ is the best example we know of this class of books. Mr. Foote's work appears to be also an extremely useful one. A great number of formulæ are given, and, as far as we have examined them, they appear to be well selected.

ART. XII.—*The Diagnosis of Surgical Cancer.* (*The Liston Prize Essay for 1854.*) By JOHN ZACHARIAH LAURENCE, Surgeon to the Northern and Farringdon Dispensaries.—London, 1855.

A PORTION of the money subscribed by the friends of the late Mr. Liston, in order to found some memorial of that distinguished surgeon, was applied in the establishment of a prize, for the best observations in clinical surgery by the students of University College Hospital. Although the Liston prize has been awarded only during the last three or four years, two of the prize theses sent in by candidates have already been published, and give us a very favourable impression of the surgical training of the successful competitors. The first treatise, by Mr. Ganjee, ‘On the Use of Starch Bandages,’* was an excellent work; and the treatise by Mr. Laurence, now before us, if it contain more doubtful points, and be liable to greater criticism, is so perhaps because the subject is one of far greater intricacy and obscurity.

We question, indeed, whether Mr. Laurence has done wisely in adopting so wide a subject as the ‘Diagnosis of Surgical Cancer’ for the subject of his thesis; or whether it might not have been better to have limited the debate to some part of the diagnosis—such as the symptoms, or the anatomical characters of the tumour respectively.

The diagnosis of cancer is considered under the following heads:

1. Diagnosis from study of the causes.

- (a) Hereditary predisposition.
- (b) Personal predisposition.
- (c) Exciting causes.

2. Diagnosis from the symptomatology.

- (a) Previous history, including the progress of the growth, and the patient's health during that time.

* Vide No. xxv. p. 184.

- (b) Condition of patient when seen, as respects local phenomena (consistence and aspect of growth, pain, hæmorrhage, discharge), and general phenomena (cachexia, wasting).
- (c) Anatomy of the growth.

3. Characters obvious to the unassisted eye, and to the microscope.

Mr. Laurence then considers each of these points *seriatim*, and refers especially to the works of Lebert, Paget, Walshe, and Velpeau, and to his personal observations, for facts. We pass over the first of these chapters in order to give, by a quotation, some idea of the mode in which Mr. Laurence treats the difficult subject of the micrology of cancer. After describing the cancer cell as it exists in its most perfect form, Mr. Laurence makes the following remarks on the fibro-plastic cell:

"It is well known that fibro-plastic cells are not uncommonly found amongst the cancer cells of well-marked specimens of cancer. Indeed, as we shall hereafter have occasion to advert to, there are on record cases of tumours which were composed exclusively of fibro-plastic elements, yet proved subsequently quite as malignant as the most marked cases of cancer. Lebert, as is well known, strenuously insists that the cancer cell and the fibro-plastic cell are two distinct entities—distinct in their anatomical relations, distinct in their clinical import. But it is not uncommon to find cells which have as much the garb of one as the other, and now and then cancerous tumours will be found composed of caudate cells, scarcely, if at all, distinguishable from some forms of fibro-plastic cells. . . . And conversely, cells are sometimes observed in non-cancerous tissues, which cells, had they occurred in a surgical tumour, would at once have been pronounced prognostic of the worst to the patient from whom the tumour had been removed. Velpeau excised a portion of the calcaneum and astragalus for caries. M. Broca found abundance of cancer cells in some of the granulations. The case did perfectly well. I met with a case of precisely the same description. Mr. Erichsen excised the head of the femur for old-standing hip-disease. In some granulations coating the carious bone, I found some cells which certainly approached most closely the 'cancer cell' in their forms and proportions. . . . Another class of facts that has presented itself to my notice is, that in several tumours, the pathology and obvious anatomy of which stamped them cancerous, while I have found typical cancerous and typical fibro-plastic cells, I have found such different gradations between these two extremes, that it was often impossible to say to which type a given cell belonged. . . .

"From the above considerations, I venture to lay down the following proposition:—*That there do exist cell-forms, which it is difficult or impossible to refer either to the cancerous or fibro-plastic type exclusively; and, as a corollary, That the existence of such forms brings us to the conclusion, that the two forms of cell cannot but be regarded as the extreme links of a chain of forms connected by intermediate stages.*" (pp. 69, 70, 71.)

Very good drawings are given of these various cells.

At the conclusion of his work, Mr. Laurence sums up his opinions of the diagnostic value of the microscopical elements, by stating that, in most cancerous tumours, the so-called cancer cell will be found; that it may also be found in growths manifestly innocent; and that growths anatomically innocent—i.e., without cancer cells—may be clinically malignant.

We shall not at present discuss these opinions, but conclude this short notice by thanking Mr. Laurence for his little work, and by begging him not to think he has exhausted the subject, but to extend and confirm his observations.

ART XIII.—*Summary of New Publications.*

WE have already noticed the majority of the works received during the three months ending February 28th. The following have now to be mentioned:

In *Medicine*, Dr. Sibson has commenced the publication of an 'Atlas of Medical Anatomy,' of the same size and price as Maclise's 'Surgical Anatomy.' One number only has reached us, but from it we have no hesitation in concluding that it will be a work of the first order, and of the greatest utility. We defer a critical examination until other parts have appeared.

Lebert has issued 'three parts of a work* on 'Pathological Anatomy,' illustrated by some magnificent plates. It is to be published in about twenty numbers, and the price of each number will be fifteen francs; the cost of the entire work will be in England about 25*l*.

A very elaborate work on 'Foreign Bodies in the Air Passages,' has been published by Dr. Gross of Philadelphia. It is a complete summary of the whole subject, and will be an useful book of reference.

An important work, demanding special review, entitled 'Eutherapeia; or, an Examination of the Principles of Medical Science,' has been written by Mr. Garner. We hope to notice it in our next number.

Dr. Black has published the second part of his 'Pathology of the Bronchio-Pulmonary Mucous Membrane.' The subject is pulmonary tuberculosis. We shall notice the paper soon.

In *Surgery*, we have received one volume of M. Roux's posthumous work;† reparative surgery forms the subject of this volume.

Mr. Harvey has written a little work of two hundred and thirty pages, on 'The Ear in Health and Disease.' It seems to contain much useful information, and is altogether a better work than the last we saw from the same pen.

The fasciculi of the second edition of Mr. Maclise's beautiful 'Surgical Atlas' are appearing regularly, and we trust meet with universal approbation.

Dr. James Arnott has written another pamphlet on 'Benumbing Cold,' which contains, however, nothing new.

In *Midwifery*, a work by Dr. Meigs, on 'Child-bed Fever,' shall be reviewed shortly; and two German Inaugural Dissertations on 'Placenta Prævia,' and on 'Hæmorrhage from the Umbilicus,' shall receive due record in the next Report on Midwifery.

In *Materia Medica* we have only received the second division of the first half of the second volume of Werber's 'Specielle Heilmittellehre,' a work of very considerable utility, but which may perhaps be completed by the time the present readers will administer no more drugs. Mr. Swan's work on 'The Brain,' Dr. Frederick Bird's translation of M. Bouchut's work on 'The Diseases of Children,' a short elementary work on Chemistry, by Dr. Gregory; and the sixth, seventh, and eighth parts of the 'Micrographic Dictionary,' complete the list of books received during the last quarter. Of these, Mr. Swan's treatise, and Bouchut's work in its new dress, will be specially reviewed.

* *Traité d'Anatomie Pathologique.* Par H. Lebert. Liv. 1—3. Paris.

† *Quarante Années de l'Pratique Chirurgicale.* Par Ph. J. Roux. Paris, 1864.

PART THIRD.

Original Communications.

ART. I.

On the Mode of Development of Tubercle in the Lungs in Chronic Phthisis: its connexion with Fatty Degeneration of the Epithelium of the Air-Vesicles, and its Early Manifestation by Fatty Epithelial Cells in the Sputa. By C. RADCLYFFE HALL, M.D., F.R.C.P. E., Physician to the Hospital for Consumption, and to the Institution for Ladies with Diseases of the Chest, Torquay.

To ascertain with precision the earliest deviation from the healthy state in the part invaded by tubercle, is obviously a point of the highest interest and importance; and the difficulty of obtaining the information, at first sight, appears to be commensurate; for, when tubercle is already present, the condition of tissue which immediately preceded its deposition may have passed away, and when no tubercle yet exists, we cannot be quite sure that any would ever have ensued. The difficulty, however, diminishes when we consider that tubercle is deposited not all at once, but progressively; that it increases by accretion at its circumference; and, consequently, that the peripheric portion of a slow tubercle must be more recent than its centre. Certain differences in colour and density are visible to the naked eye; and with the microscope we can readily discern that every tubercle of tolerable size presents a series of different appearances, as it shades off in irregular zones, from complete tubercle to healthy lung. Knowing that any given tubercle, if progressive, would have continued to enlarge by the peripheric addition of tuberculous matter, we have a right to infer that the condition of that part of it which is nearest to healthy lung represents the earliest local morbid change—the first step in that local process which ends in the formation of tubercle. At all events, we approach as nearly to the facts as the nature of the circumstances will permit. It may be objected, that any given morbid condition around a tubercle may be the mere consequence of the morbid deposit already there, and so can indicate nothing respecting the phases of primary tubercular deposition. Even then we should learn the nature of the condition which favours increased deposit, after tuberculization has once commenced. But the objection can be disarmed of much of its force, for it is only valid provided the presence of inflammation, or that of any kind of heterologous formation indifferently, is adequate to produce a similar morbid condition: if not, then tubercle coincides with this condition, not because it is a mere morbid deposit in the lung, nor because of the inflam-

mation which often accompanies it, but because it is tubercle. Now, we can show that mere inflammatory exudation, in a lung without tubercle, is not surrounded by a similar morbid zone intervening between the solidified and the healthy portion of lung; whilst tubercle which has not yet any inflammation around it, is. Then, again, we can show that a morbid deposit which is neither inflammatory nor tuberculous—as, for example, the entozoic disease in the lungs of sheep—has no such morbid zone. On the other hand, in connexion with tubercle, we find the same morbid condition in some parts of a tuberculous lung which are not yet the seat either of tubercle or of inflammation; in the portion which forms the immediate margin of the tubercle; and, in the form of relics, in the mature tubercle itself.

Thus, one given morbid condition is found where tubercle does not yet exist, but where it would probably have ensued; and around existing tubercle where we know that more tubercle would have been formed; and vestiges of the same are met with amongst the elements of completed tubercle. All this suffices to prove, at least, an intimate connexion of some sort between the condition in question and tubercle.

Development of Tubercle.—Taking for examination any tolerably large distinct tubercle, we see with the naked eye that there is no abrupt line of demarcation between the healthy lung and the margin of the tubercle. A simple lens discloses further, that the tubercle has not any distinct and even margin at all, but has a jagged, irregular outline, from processes of



Fig. 1. Preliminary stages of tubercle. From soft, grey, uninfamed lung, bordering a yellow unsoftened tubercle. + 250.

a. Epithelium, scarcely altered.

c. Epithelium, more fatty and enlarged.

b. Epithelium, fatty.

d. Compound cells.

tubercle jutting out into the surrounding lung. By means of the microscope we perceive that the edge of one of these jutting processes of tubercle is not the limit of the morbid change; but that what, on cursory inspection, appears to be not unhealthy lung, immediately bounding the real tubercle, is in reality diseased, though not tuberculized.

Tracing onwards from lung which has no abnormal appearance to the centre of a large crude tubercle, we find as follows:

1. The pavement-epithelium of the air-vesicles is more nebulous.
2. Each epithelial cell becomes enlarged, more cloudy, more prominent when seen in profile, and is studded here and there with oil-dots.
3. The epithelial cells become still larger, and more fatty. In many of them no distinct nucleus can now be made out, but large dots of oil occupy its place. Some of the cells are detached, leaving the wall of the air-vesicle in one part bare, in another coated with compound tubercle cells. These are the preliminary stages of tubercle.
4. We now arrive at the completed tubercle, which consists of compound tubercle cells, small free nuclei in abundance, and granules; and occasionally, in addition, of a few fatty epithelial cells in various stages of disintegration; all being held together by a tough matrix.
5. So far, the deposit has been confined to the interior of the air-vesicles. It now invades the intercellular tissue of the lungs. For the first time, we find tubercle-corpuscles amongst the fine fibres which form the frame-work of the walls and septa of the air-vesicles. These fibres are here and there seen to be broken into lengths, and the entire tissue of the affected lung has become the seat of tubercle. In the first stages, tubercle is only intra-vesicular; at last, it is both intra-vesicular and interstitial. But it is to be remarked, that distinctly cemented in amongst the pulmonic fibres, we never find any of the compound tubercle cells, but only the free nuclei and granules.

The steps of the local morbid process, then, appear to be these:—Fatty degeneration of previously normal epithelium; shedding of this; its replacement by fresh epithelium, degenerate from the first, and rapidly becoming fatty; shedding of this; its replacement by large cells containing several nuclei; shedding of these; their replacement by free nuclei and granules, embedded in a structureless matrix. Up to this stage the tubercle is intra-vesicular only. The pulmonic fibres are next enclosed and separated by the morbid exudation, and free nuclei and granules are formed between and amongst them. The tubercle is now complete.

Tuberculization of the lungs thus commences as a degeneration of a normal tissue, proceeds as a production of this tissue in a depraved form; next, as an exudation capable of following only the lowliest process of organization up to maturity. Its ulterior changes from maturity, also, are those of the degeneration of a lowly-organized product; but concerning these I have dealt elsewhere.*

The structural representatives of these several stages, starting from normal epithelium, are the following, and may be considered in their natural order. They may not all be detectible at once in every mature

In the Annual Address in Medicine for 1852, delivered before the members of the Provincial Medical and Surgical Association.

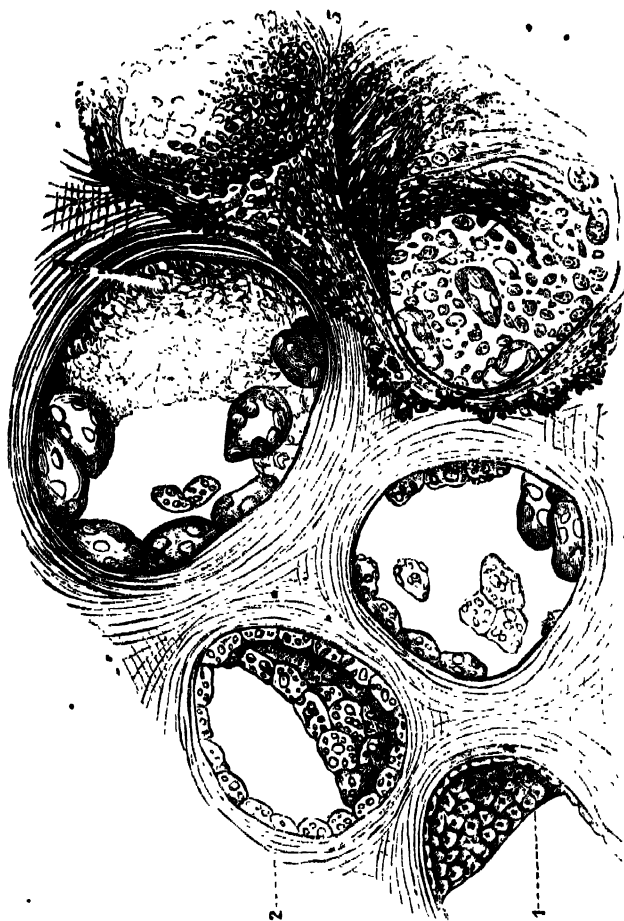


Fig. 2. Diagram of the stages of chronic tubercularization of the lungs.

1. Air-cell, with its epithelium healthy and adherent.
2. Ditto, with epithelium fatty and enlarged, still adherent.
3. Ditto, with fatty epithelium detached in places; compound tubercle cells upon wall of air-vesicle in places; wall bare in places.
4. Ditto, ditto, with exudation of tubercle-blastema into air-vesicle.
5. Complete tubercle, bounded by irregular line of black pigment consists of air-vesicles and their septa filled and infiltrated with blastema, with compound tubercle cells and fatty epithelial cells, as well as free tubercle-nuclei and granules in the air-vesicles, but only the free nuclei, granules, and black pigment interstitially amongst the pulmonary fibres.

tubercle, particularly if softening be commencing; but they are to be found in various proportions in the great majority of tubercles, provided we include along with the tubercle a certain portion of the pulmonary tissue around it.

Constant constituents of mature pulmonary tubercle at some period of its course:

1. Normal epithelial cells becoming fatty.
2. Fatty epithelial cells.
3. Many nucleated cells.
4. Free nuclei.
5. Granules.
6. Matrix.

Frequent constituents:

7. Small blood-vessels in a state of fatty degeneration.
8. Red blood-corpuscles, and orange-brown pigment.
9. Black pigment.
10. Granule cells, and glomeruli.
11. Induration-matter.

1. *Normal Epithelium becoming Fatty.*—The existence of an epithelium at all in the air-vesicles has been questioned by physiologists of high repute, but is now very generally admitted. Blood-corpuscles seen through the walls of the capillaries were indicated by Mr. Rainey as having possibly been mistaken for epithelial cells. Such an error cannot arise when the lung of a bird, or of an amphibian, is examined. The even oval outline of the large and clearly-defined blood-corpuscle is too distinctive. In the frog, the epithelial cells are coarser, dimmer, and more separated from each other, than in the bird or mammal; having reference, perhaps, to the greater expansibility of the lung-sac. In the bird, the cells run one into another at their margins, showing only a faint out-lining as their mark of division. In man also, the outlines are less sharply defined than in most other varieties of pavement-epithelium; but the flat cells are bounded by a thin line of limitation. In appearance they are thin, almost transparent, and have a slightly nebulous, somewhat ill-defined nucleus, very different from the bright sharply-cut nucleus of the pavement-epithelium of the mouth, for instance. A nucleolus is not distinctly to be made out. In size and shape the cells vary greatly; smaller and rounder when young, they become rather longer, flatter, and more angular with age. Pentagonal, hexagonal, or polygonal, with angles more or less acute or round, according to their mutual fitting into each other; their general character is, that they constitute a fine, but dimly-defined, pavement-epithelium of a single layer. Whether this internal cuticle undergoes any regular process of desquamation and reproduction in health, is unknown. In all probability, it behaves very much like the pavement-epithelium of serous membranes, in being for the most part persistent, and only cast off and renewed when accidentally incapacitated. Like serous epithelium, too, it permits the transudation of a thin watery halitus (with the especial addition, in its own case, of the gases of respiration), whilst it prevents the passage of complete blood-plasma, acting in this respect as a natural defensive coat of elastic cement.

When becoming fatty, the flat epithelial cells first appear better defined at their edge, and more nebulous at their centre. Next, they are larger, plumper, and more distinctly separate one from another, though still adherent. Oil-drops, of different sizes, spot the whole of the nucleus, which either stands out in relief as full of oil-dots, whilst the surrounding



Fig. 3. Flakes of fatty epithelium from lung adjoining tubercle, showing the various sizes of epithelium cell when becoming fatty.

portion of cell is only nebulous; or it is apparently converted into one large oil-dot; or, it is obscured or lost by general oil-dotting of the entire centre of the cell, the marginal portion only remaining clear.

2. *Fatty Epithelium degenerate from the first.*

—Nearer to the tubercle than those air-vesicles which are lined with normal epithelium, which is becoming fatty, we find other air-vesicles, containing a quantity of detached epithelium, yet still having an epithelial coat on their walls, presenting cells in various stages of fatty degeneration. Some of these are small and round, evidently young, yet fatty. In many, the nucleus is not fatty, whilst the cell is. In others, the whole is fatty, both cell and nucleus; the situation of the latter being still traceable by the clustering of oil-dots there. Lastly, the fatty spotting is so universal, that nothing remains to indicate where the nucleus had originally been. Although we have no right to infer that all these forms may not be presented by the original epithelium in different stages of fatty degeneration, it is certain that there are successive generations of epithelial cells which, almost from the period of their formation, begin to fattily degenerate, indicating a proneness from the first to fall into this kind of atrophy. In the first instance, the normal epithelium has existed indefinitely before it becomes fatty and is shed. In the last, the newly-formed epithelium is unable to maintain a normal life for more than a brief period of uncertain duration; it early becomes fatty, even whilst its growth is yet incomplete, and doubtless is rapidly shed, to make way for a fresh tribe of increasingly degenerate epithelial cells.

I suspect that those cells in which we find the nucleus alone fatty, are the degenerated original normal cells; and that those in which we see the surrounding cells largely fatty, whilst the nucleus is less or not at all so, as well as those in which all is alike affected with fatty degeneration, represent the subsequent tribes of degenerate epithelial cells. As countenancing such an opinion, I may state that I have found the earliest evidence of morbus Brightii in renal epithelial cells discharged in the urine, in which the nuclei alone were fattily degenerated.

Passing by this supposed distinction, the fatty epithelial cells, taken indiscriminately, present considerable variety in appearance and size.* The following may be given as examples (fig. 4):

a. A middle-sized, sub-angular, well-defined plate, having a large oil-globule in place of nucleus; the rest being merely nebulous near this oil-globule, and clear near the circumference.

b. A rounder cell, either small or middle-sized, having a large oil-globule in place of nucleus, and remainder of cell dotted with oil-dots of different sizes.

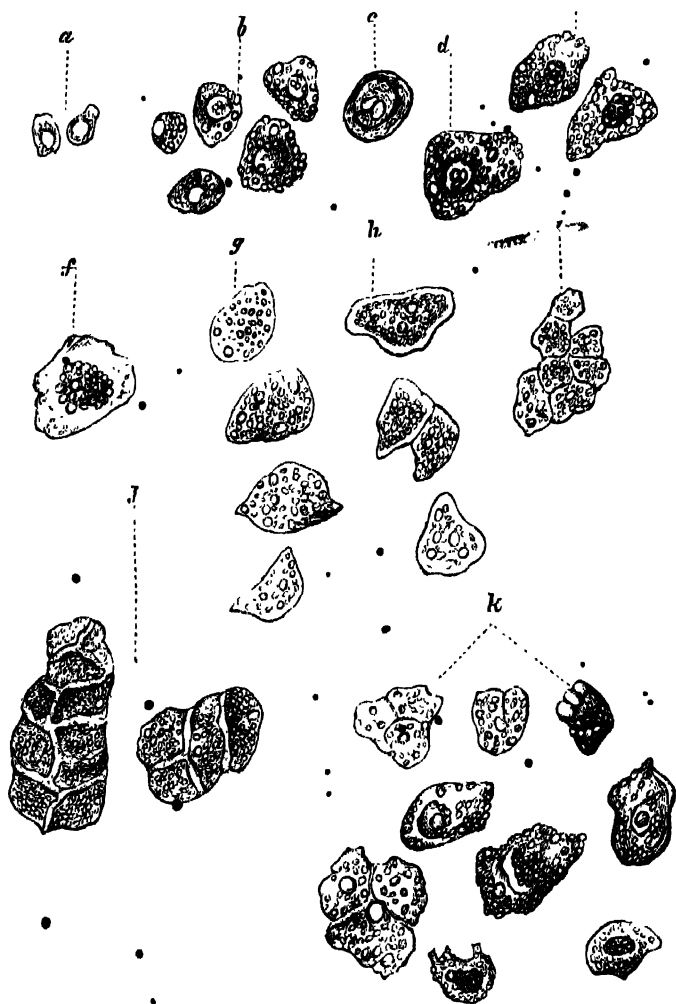


Fig. 4. Various forms of fatty epithelial cells, single and in flakes, as described in the text.

c. A flat cell, not generally fatty, containing two or three oil-globules in its centre.

* *d.* Flat-cells of very irregular outline, having a large nucleus with central oil-dots; rest of nucleus clear; cell around nucleus full of oil-dots of varying size.

e. The same as the last, but whole of nucleus fatty, though still discernible.

f. An irregular cell, not at all fatty, but having very large nucleus dotted all over with fat dots.

g. Flat cells, of various but irregular outline, wholly fatty; nothing in place of nucleus detectible. Oil-dots very variable in size.

h. The same as the last, but with a margin of cell all round, not fatty, but dimly clear.

i. Flakes of detached epithelium, showing the cells altogether fatty, having a sub-angular outline, still adherent at their edges; very varied in size and shape, but fitted one into another without any interval of separation.

j. Similar detached flakes, but the fatty epithelial cells more swollen, and, though still coherent, showing a linear interval of separation from each other, resembling in this particular the lung of the batrachian.

k. Fatty epithelial cells, apparently disintegrating.

In size, the fatty epithelial cells vary so considerably, that no standard can be given as the average. A very few may be found smaller than a medium-sized normal lung-epithelial cell: by far the majority are larger, and some much larger. The smallest and the largest are generally the least angular in outline; the one perhaps from youth, the other from distention. Still, singularly-shaped cells, with abruptly cut margins, seeming to indicate that the cell has been partially disintegrated, are seen of all sizes.



Fig. 5. Bronchial columnar epithelium, from lung adjoining yellow tubercle. + 450.

Bronchial columnar epithelium is found freely in the several conditions of withered but not fatty; fatty and swollen; partly fatty and partly withered. In some of these detached bronchial epithelial cells the nucleus alone is fatty; or the columnar part alone is fatty, the nucleus being merely granular; or the whole is made up of oil-globules of different sizes, no nucleus being left.

Of course, more might be observed on the minute differences presented, but the above may suffice to establish the fact that what is thus described really is epithelium in a state of fatty degeneration, and not any of the morbid cell-forms which result from inflammation, and are generated in inflammatory exudation. In answer to the first question which is usually put, How are these cells to be distinguished from the granule cells of inflammation? it is to be remarked, that the easiness of making the distinction depends entirely upon the individual cells selected. A young gland-cell in one viscus is very much the same in appearance as a young gland-cell in another, however clear may be their ultimate dis-

tinctiveness at maturity. So, also, of pathological cells: one may find some specimens in every diseased product not distinguishable from others belonging to a very different disease. We are ruled by the form of mature cell which preponderates, and take that for the type. So here, by the same rule, we find certain cells which are so manifestly epithelial cells more or less fattily degenerated, and we can trace such marked gradations in them, that we need feel no hesitation about our conclusion because a few of the cells, if taken alone, could not with certainty be placed in the same class. There is no room for doubting, for instance, the nature of a flake of pavement-epithelium in which the cells still cohere and fit in by their edges, whilst they present various degrees of fatty degeneration. Neither can any doubt exist when we are viewing fatty columnar epithelium from the adjacent bronchi.

3. *The compound Corpuscles of Tubercle, or many-nucleated Cells.*—These are large cells, which contain several separate nuclei. We do not find many of them in the mass of mature tubercle, and what there are lie here and there, and not in aggregated heaps. Neither do we find them in those air-vesicles which are nearest to healthy lung. Here the epithelium is only fatty. But between the air-vesicles which contain the fatty epithelium and those which are crammed with complete tubercle, these cells are numerous.

They were first described by Virchow in 1851, and by Van der Kolk in 1852. The former mentions "cells with five large, oval, granulated, nucleolated nuclei." The latter states that the cells nearest the wall of the air-vesicle are the smallest, contain only one nucleus each, but when cast off increase greatly in size by the imbibition of fluid, and are filled for the most part with numerous nuclei. "The cells which are placed in the middle of the air-vesicle are thus the oldest—i.e., they are furthest removed from its walls, longest exposed to the influence of the surrounding fluid, and thus, also, the largest." By both observers these cells are considered to be morbid epithelium; and by both the contained nuclei are believed to be set free by dissolution of the enveloping cell, and then to constitute the small cells described by Lebert and others under the name of tubercle corpuscles. For the further interpretations, somewhat differing, of the two authorities, I must refer to the only sources of my own acquaintance with them—the accounts severally given by Dr. Jenner* and Mr. Paget†.

My own observations lead me to conclude—(1.) That the many-nucleated cell may be found of the largest size, and containing its largest number of nuclei, whilst close and adherent to the wall of the air-vesicle; (2.) That no successive strata of cells, becoming more and more nucleated as they advance free from the wall towards the centre of the air-vesicle, can commonly be made out; (3.) That no distinct lamination of cells of any kind upon the wall of the air-vesicle, as if they had been thrown off in successive and distinct layers, is observable; (4.) That cells equally large, equally centric as regards the air-vesicle, are numerous, in which either only one nucleus, or none at all, is discernible, the whole being in some stage of fatty degeneration; (5.) That a rather small cell may contain several

* British and Foreign Medico-Chirurgical Review, vol. xi. p. 183. 1853.

† Ibid., vol. xii. p. 196; and Lectures on Surgical Pathology, vol. ii. p. 595.

nuclei, whilst one much larger, as just remarked, may have only one; (6.) That whether or not the nuclei of the many-nucleated cells, if set free, would be identical with the free tubercle-corpuscles, they are not the principal, still less the only, source in which these originate.

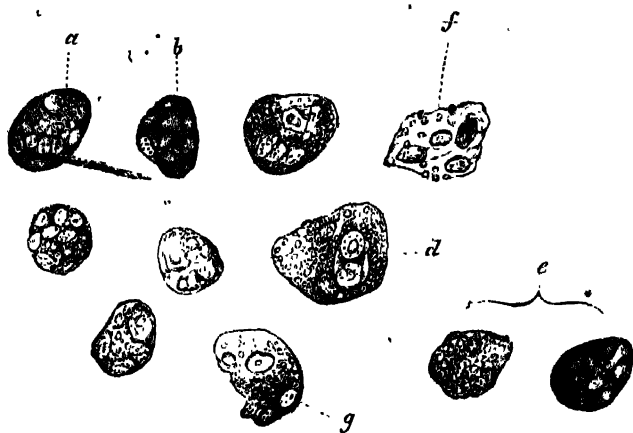


Fig. 6. The many-nucleated tubercle cells. + 450.

- a. A plump cell, nuclei rather dim.
- b. Another with one lateral nucleus, very distinct, and closely resembling a free tubercle-corpuscle.
- c. An aggregation of coherent nuclei, but no distinct parent-cell-wall around them.
- d. A large cell, having a second cell enclosed with two nucleated nuclei, taken from a highly organized tubercle—the grey miliary, with fibrillated matrix.
- e. The same cell at different foci.
- f. An apparently shrivelled compound cell.
- g. A compound cell, partially disintegrated.

The many-nucleated cells, or compound corpuscles, are darker than any other of the cell-forms seen in examining tubercle. They have often a clouded, yellowish-brown aspect, and at first glimpse are readily mistaken for large glomeruli. They are usually plump and roundish, ovoid, or pyriform, and strikingly differ in appearance from equally large, but flatter and more or less angular, epithelial cells which are only fatty. Their nuclei lie at different depths, and cannot all be seen distinctly at once. Distinct and in sharp relief, indeed, it is only by chance that any of them can be seen, owing to the dense nebulousness of the cell in which they are contained. So far as can be distinguished, these nuclei, whilst within the cell, are ordinarily larger, plumper, and more regular in outline than free tubercle-corpuscles; they look less hard and compact, and their granules less distinct. Occasionally, however, we catch sight of one close to the wall of the containing cell, which appears to resemble closely an ordinary free tubercle-corpuscle. And, in examining large miliary tubercles, grey throughout, I have sometimes seen a faded, shrivelled, flat, semi-transparent cell, no longer granulous, in which lay three distinct very characteristic tubercle-corpuscles. (Fig. 6, f.) This I took to be a compound cell, on the eve of dissolving and setting free its nuclei. It is not unfrequent for one of the nuclei in a compound cell to be larger than the rest, and to possess a nucleolus. There may even be two nucleolated nuclei side by side in an oval cell, contained within a large com-

pound cell having the ordinary non-nucleated nuclei. . This form is rare. I have only found it in the highest type of tubercle—viz., the grey miliary, with fibrillated matrix. (Fig. 6, *d.*) Usually, there is only one nucleolated nucleus in a compound corpuscle, however numerous the fellow nuclei may be. This nucleolated nucleus may be supposed to represent the original nucleus of the epithelium cell; the remaining non-nucleated nuclei being formed secondarily, as the cell increases in size, from mere corpusculation of plasma imbibed whilst the cell is still adherent to the wall of the air-vesicle. Whether such growth of cell, and multiplication of nuclei within it, also go on after detachment from the wall of the air-vesicle, cannot, I believe, be decided either way by anything we can find on inspection. If the cell can grow after severance from the place of its birth, or if it be, from the time of its detachment, large enough to swell out by mere imbibition, there is no reason why the imbibed plasma should not corpusculate as readily on the inside as we find it does in the matrix on the outside of the cell. There is no ground for supposing, in any case, that the several nuclei result from fission of the primary nucleus, since that, as already noted, is very often still present and increased in size. I suspect that most of the compound cells complete their development whilst still adherent, and only increase in growth subsequently.

This compound cellulation is by no means peculiar to tubercle. In a more marked form, it is common in cancer; but, in very similar form, it may be met with in the cheesy secretion of the tonsils—in that of a sebaceous follicle—in healthy granulations, both in man and animals (on a horse's broken knee, for instance)—and in the plastic exudation surrounding the entozoa so constantly present in the lungs of sheep. Neither is it essential to tubercle. In a thoroughly tuberculous subject, in the midst of a thick adhesion, which connected the base of the left lung in front to the diaphragm, I found three triangular portions of true adipose tissue; and in the centre of each a distinct crude tubercle. In each

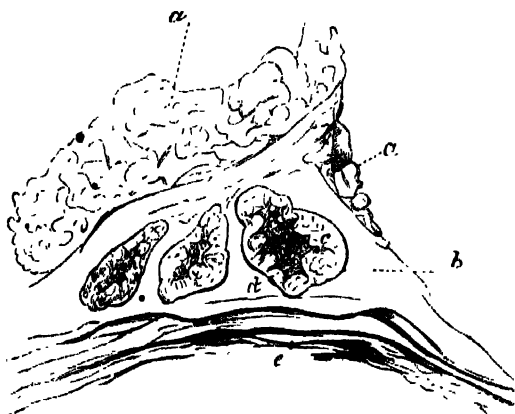


Fig. 7. Three tubercles in an adhesion between lung and diaphragm. Natural size.

a. *a.* Lung.
c. Fat.

b. Tough, firm, pinkish, gelatinous adhesion-matter.
d. Tubercle.

e. Diaphragm.

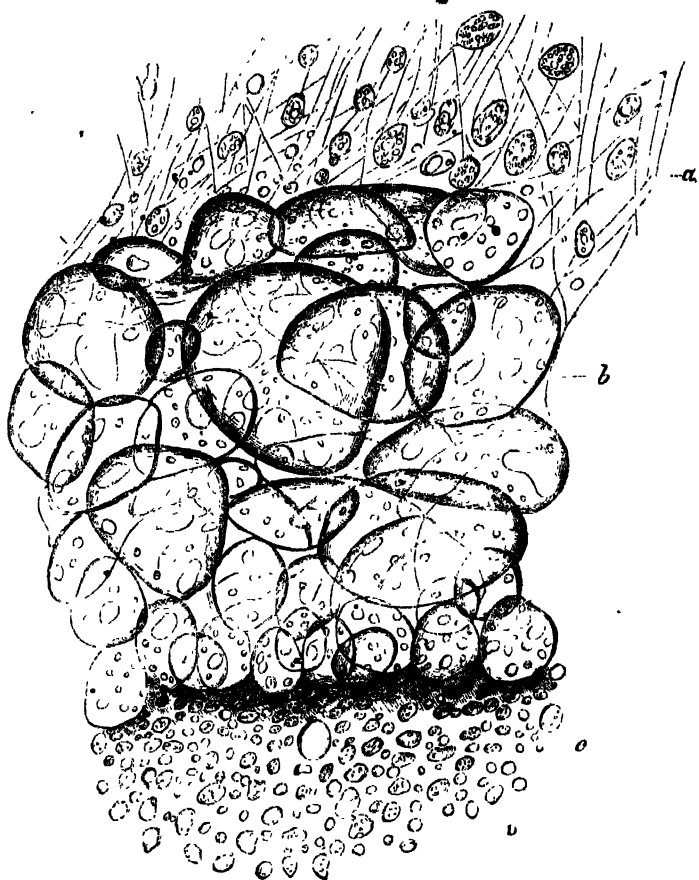


FIG 8. Section through adhesion and fat-wall into one of the tubercles. + 250.

a. Adhesion-matter.

b. Adipose tissue.

c. Tubercle, containing fat globules, free tubercle nuclei, no compound cells, and but few granules.

of these tubercles, characteristic free corpuscles and granules set in their matrix, formed the entire mass. There was not a compound corpuscle in any one of the three tubercles. Evidently, the course of events had been this:—The lung, having been made an abnormal fixed point, by adhesions and tuberculous consolidation occurring at the last stage of phthisis, the diaphragm had had a tendency to drag asunder the newly-formed adhesion-matter. As this opened out into areolar tissue, fat was deposited; and into this loose fatty areolar tissue, plasma subsequently found admission, and corpusculated *more suo* into yellow tubercle. There was no epithelial surface engaged, and there were consequently no compound cells. So, likewise, when pulmonary tubercle has become interstitial, we do not find the compound tubercle-corpuscle amongst the lung-fibres. Neither do we find it in sub-peritoneal tubercles. As Virchow (and

also, I believe, an English pathologist of equal eminence) has found the many-nucleated cells constantly in tuberculous lymphatic glands,* we may perhaps infer that, although not essential to tubercle, and consequently not the crucial fact of its occurrence, this compound cellulation is still an habitual feature of tuberculization at some stage *when it attacks an epithelial surface*, such as that of the lungs and of the lymphatic glands. That this circumstance is due rather to the accident of place, than to the kind of morbid crasis on which the disease depends, is deducible again from the fact, that the typhus-matter in Peyer's glands occasionally presents a similar phenomenon, amongst its several other points of resemblance in microscopical appearance, to the lower forms of tubercle.

It might be a question whether all of the compound corpuscles of tubercle do originate as described, acting as parent cells to endogenous nuclei. Whether in some of filmy appearance, and which are devoid of any nucleolated nucleus, the free nuclei were not the first in the field, becoming secondarily enveloped in a film of plasma whilst in contact with the wall of the air-vesicle: just as we see shrivelling blood-corpuscles become thus encased on their way to form pigment cells.

We may fairly assume that the formation of the many-nucleated cells, whether it take place in one, or other, or both these ways, implies the exercise of more organizing power, and therefore the presence of greater vitality, in the parts, than does the production of free nuclei only.

4. *Free Nuclei, or Tubercle-Corpuscles Proper.*—These are the small single cells, which make up the bulk of every variety of mature tubercle. In appearance they are irregularly round, oval, or oblong; or bean-shaped, or polyhedral, or altogether irregular; but always without sharp corners; their shape varying apparently according to their age, the kind and condition of tubercle, and the degree of pressure from close packing to which they have been subjected. They are more regular in figure in the jelly-like tubercle occasionally found in the most acute forms of phthisis, in grey miliary tubercle, and in yellow tubercle just beginning to soften; and most rounded and plump when softening has fairly set in. Least regular, in friable yellow tubercle, when it is commencing the process of dry obsolescence. They have a sharp, compact outline, are semi-opaque, and contain several very distinct dispersed granules, but, commonly, no separate and definite nucleolus. In size, they vary very considerably, from that of a very minute cell, specked with molecules, up to that of a pus-corpuscle. The great majority are rather smaller, regard being had to their constant difference in figure, than a red blood-corpuscle. They are consequently the smallest of the typical pathological cells;—exudation granule cells, pus cells, and cancer cells, *when fully developed*, being usually much larger than these tubercle-corpuscles. Those are also, all of them, nucleated cells; the unnucleated specimens being the exceptions. Tubercle-corpuscles, on the contrary, as a rule, are not nucleated (or nucleolated, if we name the corpuscles themselves nuclei); those which are so, are the exceptions. As they have not the size, neither have they the plump rotundity of most of the other morbid cells in their perfect state. Still, the tubercle-corpuscle is not a shrivelled, withered-looking cell; but gives one the impression of its being a small, tough cell, which has grown up to its present size and figure, and could not grow beyond it; and not of a cell which

* My own observations have been almost limited to tuberculized bronchial and mesenteric glands. In these, I have always found some compound cells.

has passed through a higher stage of development, and is now going down by some kind of degenerative process. In this respect, it differs entirely from shrivelled-up inflammatory granule cells, or old pus-cells. These may have assumed the small size of the tubercle-corpuscles, and have lost their roundness; but then they look shrivelled, and have an uneven irregularity of outline; whereas the tubercle-corpuscle, whatever its irregularity of outline, is even, and shows distinctly the few rather large granules within it, and never looks muddy or clouded with minute molecules. Moreover, after swelling them out with water, and using diluted acetic acid, a nucleus can ordinarily be made out in the other cells, however shrunken they may be; it cannot, for the most part, in tubercle-corpuscles.

The granules contained in a tubercle-corpuscle, prior to the period of softening, cannot be made to move about within the corpuscle by any mode of manipulation. It is hence inferred that the corpuscle is filled, not with fluid, but with solid matter, which is identical, in all probability, with the external matrix.

Tubercle-corpuscles may be defined as—small, irregular-shaped, well-defined, semi-opaque, motionless-granule-bearing, unshrivelled, non-nucleated cells. Only one other kind of morbid cell could be included under this definition, and that is the cell more sparingly found in typhus-deposit.

Nucleated Cells.—Cells which contain a distinct nucleus are found more or less abundantly in many tubercles. The following varieties may be noticed:

a. A small roundish or oval cell, neither granulous nor fatty, having a bright oval vesicle in its centre.

b. Cells containing distinct granules, and only distinguished from the common tubercle-corpuscles in that they are longer and more regular in shape, and present a round or oval nucleolated nucleus.

c. Smaller cells than the last, bearing distinct granules, and in every respect like the free tubercle-corpuscles, excepting that a bright vesicle appears within them, either at the centre or nearer one end. (Fig. 9.)

Are these and such-like nucleated cells different stages of morbid epithelial cell, or of the tubercle cell, or of some superadded inflammatory granulous cell, or of modified pus?

That they are neither inflammatory globule nor pus, is clear from their presence in unsoftened tubercle, and most abundantly in grey miliary tubercle, which has no inflamed lung around it. Before stating what they probably are, we must inquire into the mode of origin of the free unnucleated tubercle-corpuscles. These are referred by Virchow and Van der Kolk to the disintegration of the many-nucleated cells having set free their nuclei, of which they are consequently the parent or brood-cells.



Fig. 9. Free tubercle-corpuscles. + 450.

a, b, c. Varieties of nucleated cell, as described in the text.
d. Non-nucleolated nuclei.
e. Free molecules.

Some of the free corpuscles may originate in this manner, and may hence, with propriety, be designated free nuclei; but all, and probably the greater number, can scarcely so originate; for, we find the characteristic tubercle-corpuscles in masses in which no many-nucleated cells exist, and, as far as can be ascertained, never did exist—e. g., in the instance of tubercle in adipose tissue, already mentioned (figs. 7 and 8), and in the interstitial part of the deposit in pulmonary tubercle (fig. 2), and in sub-peritoneal tubercle;—and we also find miniature specimens in all degrees of the free corpuscle, smaller than we can ever make out amongst the nuclei contained in the large compound cells. These are not to be looked upon as young cells which would afterwards have developed into larger free nuclei, but as such nuclei made very small at first.

I would suggest the following explanation. The vitality of exuded plasma is partly inherent, partly dependent upon the adjacent living structure. When tuberculous plasma is exuded upon a surface whose normal office is that of forming epithelium, its subsequent collulation follows, as far as its own defective capability permits, the type of epithelial cell-formation; presenting, as the result, numerous aberrant forms of nucleolated-nucleated cell, or merely nucleated cell. When such a cell is small, and contains a few granules, it constitutes what has been described as “a nucleated tubercle-corpuscle.” But when tuberculous plasma cellulates at a distance from the wall of the air-vesicle, it forms only that kind of lowly cell which its own unassisted capacity permits,—and that is, the unnucleated tubercle-corpuscle. And when, under the progress of disease, the pulmonary tissue has had its vital force too much impaired to afford anything towards organizing tuberculous plasma into its higher corpusculate forms, then, also, the exudation, although close to lung-tissue, may generate only the unnucleated corpuscles. Moreover, as all plasma will corpusculate when exuded, and as the resultant corpuscles will tend towards nucleation, in proportion to the goodness of the plasma, it is possible that some portion of the plasma poured forth in a phthisical subject may have the capacity within itself of forming small nucleated cells amongst the elements of tubercle, irrespective of any influence of the pulmonary tissue. In either case, the presence of nucleation may be taken to indicate a higher measure of vitality, or organizing force, in the tubercle cells which possess it than in the rest.

Such of the nucleated corpuscles as may originate in the way we assume, are not retrograde, nor degenerating, nor even tubercular epithelial cells, strictly speaking, because they have never been developed higher than we now see them. But at their origin, they may be said to have inherited a tendency towards the type of epithelium, although they never fulfil it.

Other tubercle cells may be nucleated in virtue of inherent capacity of plasma; these, probably, are the rarest. Some, again, are probably nothing more than the nucleolated nuclei of former compound corpuscles set free by disintegration. The diversity in appearance presented by the different forms of nucleated cell in tubercle, quite countenances the supposition that there may be several modes of origin for them.

The nucleated cell is not essential, or even in any sense characteristic of tubercle, for it is not always present; and seldom abundantly so, except

in certain specimens of fibrillated grey tubercle. But it does not follow that when present, it is not equally one of the elements proper to tubercle as any other.

The small non-nucleated cells, free nuclei, abortive cytoblasts, or tubercle cells proper, may be looked upon, with Lebert, as the especial characteristics of tubercle. In variable proportion, they are to be found in every tubercle; and if they were not also met with in typhus-deposit, might be considered as the pathological element which was peculiar to tubercle. In typhus-deposit, however, these corpuscles do not constitute the bulk of the exudation; molecular detritus in abundance, a few compound cells, and a few of these free nuclei, are mixed up together in a less firm matrix. Whereas in many tubercles, these corpuscles, set in a tenacious matrix, are the predominating elements.

Of these tubercle-corpuscles, some are probably nuclei which were formerly contained in a brood-cell now dissolved away; but the majority have been self-originated in an exudation of tuberculous plasma. Excepting tuberculous plasma, no other kind has the tendency to generate in abundance free unnucleolated nuclei like these. In common inflammatory exudation, we may find a few young cells not nucleated, but they are either round, or oval, or shrivelled; and in degenerated inflammatory cells, as already remarked, a nucleus may generally be detected. In cancer, here and there, a free nucleus might be undistinguishable from a tubercle-corpuscle, but there are always many other cell-forms in greater quantity. Any deposit, of which the greater portion consists of free nuclei answering to the description given, may be justly considered to be tubercle.

Whatever may be the resemblance between various other morbid deposits, when in a state of degenerative change, and tubercle; in their typical condition, all of them can be distinguished from tubercle, and *vice versa*. There is, therefore, no evidence whatever, that any exudation which was not stamped as tuberculous at the first, can ever change into true tubercle subsequently. It may certainly soften and lead to ulceration, and pursue, in other respects, much of the course of tubercle, and end in the same way; but all that does not establish identity of nature.

5. *Granules*.—The free granules are too minute for any structure to be discerned in them. They are irregular in shape and size, but always exceedingly small, and are probably nothing more than aggregations of atoms, without any definite arrangement. From their reactions, it is concluded that some are fatty, others albuminous in composition. They are comparatively few in grey miliary tubercle; in the ash-coloured, jolly-like tubercles of some cases of acute phthisis; abundant in the yellow tubercle of acute phthisis; in cheesy tubercle generally; and in destructive softening in all stages.

Are these free granules the detritus of pre-existent corpuscles, or primordial elements of tubercle? Probably both. As they are less abundant in young, and more abundant in old, degenerating tubercles, there is some reason for viewing them as in part the remains of corpuscular elements which have undergone disintegration. On the other hand, they abound in one degenerate form of tubercle at its earliest stage—viz., in the small yellow deposits of acute phthisis, where there has not

been time for corpuscular disintegration to have taken place; and where we must consider the free molecules as primordial. We find, moreover, in this case, both the albuminous and the fatty molecules, and cannot therefore view the one variety, rather than the other, as the result of disintegration. So much of the plasma as can neither corpusculate into cells, nor coagulate into mere matrix, nor be absorbed, probably granulates into these minute molecules.

6. *Matrix*.—If we move about the tubercle corpuscles under the microscope, we see that they are held together by something which is transparent, homogeneous, structureless; which, in the yellow tubercle presents no appearance of fibres or of fibrillation, and has no adherent nuclei; but, in the well-defined grey semi-transparent tubercle, is finely fibrillated. In the mature tubercle, this matrix is firm and solid; in the small grey miliary tubercle, it is as resistant as foetal cartilage; in the recently-formed tubercle of certain cases of acute phthisis, it is less firm, and is sometimes found of the consistency of tough jelly; and in softening tubercle, the matrix gradually loses its tough consistency, and finally becomes liquid.



Fig. 10. Fibrillated matrix, containing nucleated and non-nucleated tubercle-corpuscles, from grey miliary tubercle.

7. *Small Bloodvessels in a State of Fatty Degeneration*.—I have hitherto found these once only in large grey semi-transparent fibrillated miliary tubercles, which lay as distinct nodules in uninfamed lung.* Near to the portions of fattily-changed vessels, shrivelled red corpuscles, and orange, as well as black, pigment, lay amongst the tubercle corpuscles in the tenacious fibrillated matrix. The tubercles were of larger dimensions than miliary tubercles ordinarily are, averaging the size of duck-shot, and including, of course, within each many air-vesicles. The fatty blood-vessels were unmistakable, presenting a clear, defined outline, dividing into branches of considerable length, and having no openings or connexions with the adjoining elements. The centre of the vessel was bright and translucent, and the whole was studded with dim granules and oil-dots of various sizes. The patient had suffered from copious and repeated hæmoptysis two years before his death.

Considering how many cases of phthisis spring into noticeable activity coincidentally with an attack of hæmoptysis, the patient so commonly declaring that his chest was strong and his health good until suddenly he "broke a bloodvessel," which led to consumption; this fact of fatty degeneration of small bloodvessels, where no inflammation exists, at the earliest stage of the highest form of tubercle, whilst as yet it occasions neither pain nor organic irritation of the lung, is valuable and explanatory. No doubt such a degenerated vessel, under some temporary muscular exertion or mental excitement, does literally "break," and the flow of blood is then a veritable hæmorrhage from a patulous vessel, and not a mere transudation, like that through the mucous membrane of the stomach in coffee-ground vomit. If so, the popular phraseology is correct.

I have never found bloodvessels in a yellow tubercle; and these fattily-

* I have also found fattily-degenerate bloodvessels in the walls of large cavities; and occasionally shreds of the same amongst the contents of closed cavities.



Fig. 11. Small bloodvessel in the state of fatty degeneration, from large grey miliary tubercle, within which the walls of the air-vesicles were still detectible.

degenerate vestiges in certain grey tubercles evidently represent the blood-vessels which originally belonged to the lung, and not vessels newly formed because of, or in any sense belonging to, the tubercle.

8. *Blood Corpuscles, and Orange and Brown Pigment.*—Red blood-corpuscles unchanged are not common in tubercle; but a few shrivelled ones enclosed singly, or in small clusters of three or four, in a filmy envelop, are usually to be found. Yellow, orange, and brown cells may be traced to consist of such cellulated blood-corpuscles undergoing solution, and changing colour as the disintegration proceeds.

9. *Black Pigment* in granules, either free or else contained within a nucleated cell, is common both in and around tuberculous deposits, and abounds in direct proportion to their chronicity. Seeing the gradual alteration in the appearance of the enveloped red-globules last mentioned, and noting that black pigment is normally found on vascular surfaces, where there appears to be a special provision for rendering the circulation sluggish (e.g., the black pigment on the choroid, with its *vasa vorticiosa*); and, abnormally also, chiefly in parts which are very vascular, and in which there is a tendency to obstructed circulation (as in tuberculized lungs, infarcted bronchial glands, and soft cancer which does not discharge its blood by external hæmorrhage); it is difficult to avoid the conjecture that altered red-globules constitute the material out of which the black granules are formed by a process of disintegration, and subsequent molecular attraction, after their enclosure in a cell. This pigmental degeneration may be taken to indicate chronicity in the disease, and tendency to local stagnation of blood.

10. *Granule Cells.*—The large round or oval cells, full of small similar-sized granules, so constant as a consequence of inflammation, are very common in and around tubercle. They abound in exact proportion to the extent of inflammation, of which, indeed, they are a principal measure. The only form of tubercle-cell with which, when very large, they may readily be confounded, is a compound corpuscle, too granulous for its several contained nuclei to be easily distinguished.

11. About and around the seat of tubercle, the lung-tissue is generally more or less consolidated by induration-matter. Tough, glazy, and firm, it presents, variously amalgamated, the nuclei, nucleated cells, and fibres of common plastic induration-matter, together with the characteristic free nuclei of tubercle.

Other constituents of tubercle and its boundaries are met with, but they belong to the ulterior changes which constitute the conservative and destructive courses of tubercle, and are elsewhere more fitly noticed under those heads.

Sputa.—If such be the real course pursued by the local disease, we shall naturally expect to find some evidence of fatty epithelial cells being cast off from the air-vesicles in the expectoration at the commencement of phthisis. This corroboration is not wanting. I have made careful examinations of the sputa in cases in which the deposition of tubercle appeared threatening, but in which no auscultatory proof was detectible; in cases of phthisis in its earliest confirmed stage, and in others where the disease was of old duration, and in its third stage. The following is the sum of these observations on the sputa in impending, incipient, and established phthisis.

Common to all.—Flat pavement epithelium, with sharp clear nucleolated nucleus, entire or partially dissolved, from mouth or pharynx. Bronchial columnar epithelium. Filmy cells, containing two or three dim granulous nuclei from tonsils. Pigment cells.

Impending Tuberculization of Lungs.—Expectoration grey, glairy, adhesive; brought up chiefly when dressing in the morning, and scarcely noticed or thought of by the patient. If innocent (which of course it generally is), it consists merely of round nebulous mucous corpuscles, in transparent adhesive mucus. If threatening, it contains, in addition, single plates, or small flakes of flat epithelium from the air-vesicles, fattily degenerating; and bronchial columnar epithelium, also presenting various degrees of fatty degeneration.

Incipient Tuberculization.—The same as the above; and, in addition, red blood-globules, more or less shrivelled and faded, enveloped in a filmy cell; a few large many-nucleated cells; granules; and frequently small casts of air-vesicles and the ultimate bronchi, in which are plainly visible epithelial cells in various sizes and various stages of fatty degeneration. So constantly have I met with enveloped blood-corpuscles in sputum which betrayed no appearance of blood to the naked eye, that I suspect that this microscopic hæmoptysis is seldom, perhaps never, altogether absent in those cases of commencing phthisis in which the more obvious expectoration of blood is wanting.

Established Phthisis.—The same as the last, largely mixed up with corpuscles of pus and mucus. Free tubercle-nuclei occasionally in small quantity.

Large Suppurating Cavities.—Absence of specific tubercle-cells, excepting portions of opaque tubercle itself happen to be expectorated. Habitually, little else than pus and mucus, and large granulous cells, unless coniferoid vegetations be superadded, which is not unfrequent.

The microscopic appearances presented by the expectoration are most special and characteristic in the early stage, when other physical evidence

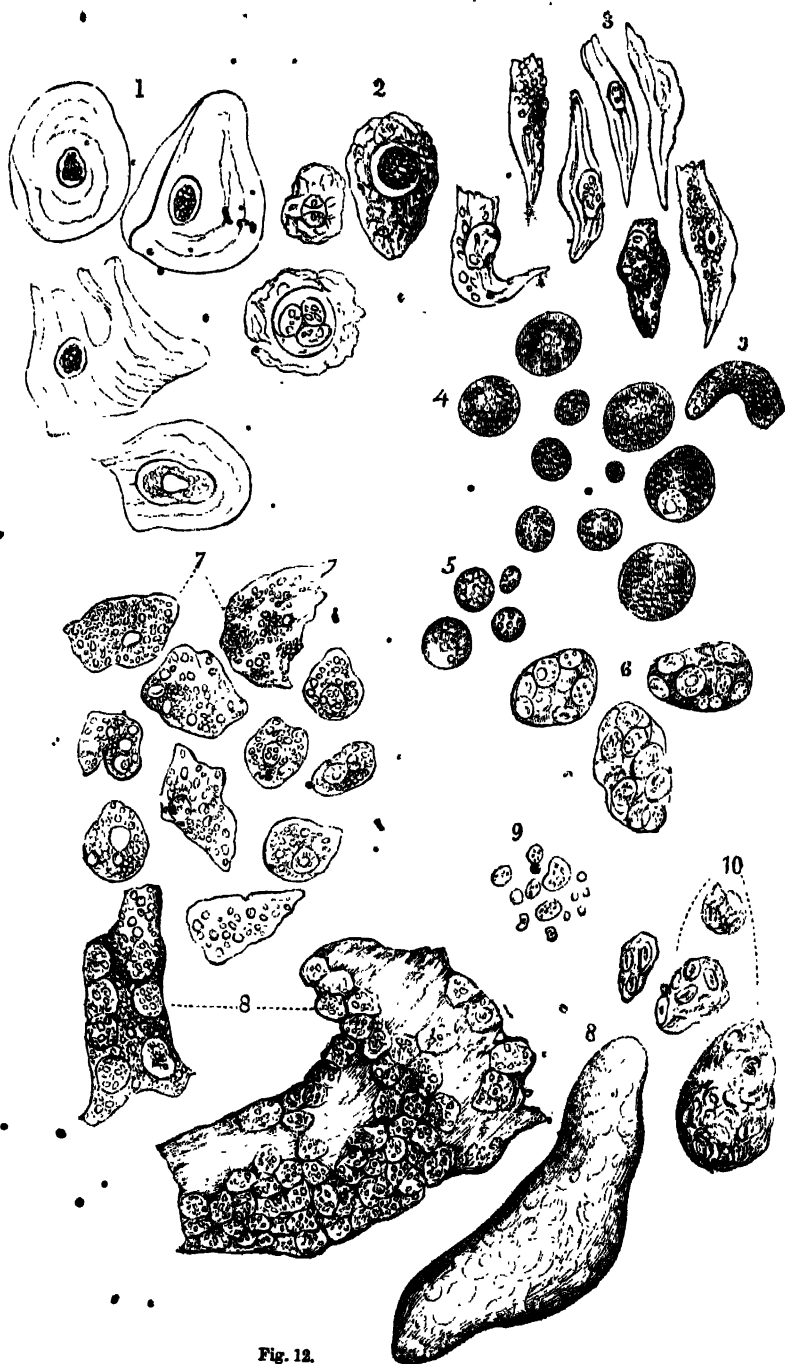


Fig. 12.

Fig. 12. Expectoration in phthisis.

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|---|--|
| 1. Oral epithelium. | 2. Filmy cells from tonsils. |
| 3. 3. Bronchial epithelium, some fatty. | 4. Mucous and pus cells. |
| 5. Pigment cells. | 6. Many-nucleated tubercle cells. |
| 7. Fatty epithelium from lungs. | 8. 8. Filmy casts. |
| 9. Free tubercle nuclei. | 10. Red blood-globules, enclosed in filmy envelopes. |

is least marked; and may be re-enumerated as consisting of fatty epithelial cells, single or in patches, fatty bronchial cylinder epithelium, casts of air-vesicles, lined with fatty epithelium, and microscopic hæmoptysis—i.e., enveloped red blood-globules. These are enough to indicate that consumption is threatened. When the many-nucleated cells and special free nuclei of tubercle are superadded, no doubt can remain. All these I have repeatedly found in cases where the patient was not apprehensive of phthisis, but complained only of general mal-aise or dyspepsia, as well as in other cases in which consumption was feared, but was not supposed to be yet in active existence.

Theory of the Local Part of the Disease.—The natural defensive epithelial coat of the air-vesicles is unable to maintain its own nutrition, and slowly falls into a state of fatty degeneration. It is then thrown off, and is succeeded by fresh epithelium, which is badly organized from the first, and quickly becomes degenerate and shed. This process of fatty degeneration and shedding may, perhaps, go on for some considerable time without further local disease; and we do not know but that it may happen in occasional instances, where the persons affected have their general nutrition by some means so much improved at this juncture, that good epithelium is once more laid down. This is quite conjectural; but if it ever do occur, such persons have been on the eve of becoming tuberculized, but have escaped. Ordinarily, after an uncertain number of desquamations of fatty epithelial cells, the surface of the air-vesicle forms cells which contain separate granule-bearing nuclei. In due course these also are shed and probably some may grow larger after their shedding. A variable number of crops of these compound corpuscles may succeed each other, according as the wall of the air-vesicle maintains, for a longer or shorter period, sufficient organizing power to make them. The wall eventually becomes bare in places, and the albuminous part of the *liquor sanguinis* has no longer anything to hinder it from escaping into the air-vesicle. It does so escape; coagulates and corpusculates into such small, compact, and for the most part non-nucleated, cells, as its feeble organic force permits; and these are the free nuclei, or tubercle-corpuscles proper. That portion of exuded plasma which is unable to perform even this feeble act of development, falls into amorphous granules. The adjoining air-vesicles, under a continuance of this process, are crammed with desquamated cells, and with tuberculous exudation. By the resultant pressure, the remnant of vitality which the enclosed intercellular pulmonary tissue (*viz.*, the septa and walls of the air-vesicles) retained, is still more impaired; exudation infiltrates amongst the pulmonic fibres, and corpusculates in their interstices into the free nuclei, but not into the many-nucleated cells. The tubercle is now complete. Up to this time it may have been unattended by inflammation, or inflammation may have attended its course throughout.

The relation of inflammation to tubercle is far too important to be

cursorily dismissed, and is discussed at length elsewhere.* I will merely state dogmatically here that tubercle *may* pass through all its stages *except the destructive ones*, without the coincidence of anything which we can justly term inflammation. But, on the other hand, it seldom does. Tubercle may commence with inflammation, or be quickly followed by it. Inflammation, in short, may step in at any stage, and almost invariably does step in at some stage, and is a necessary antecedent to the fatal issue of softened tubercle. Inflammation practically, therefore, exercises the greatest influence upon the course and termination of tubercle.

If we choose to say that when pulmonary tubercle commences with pneumonia it has an inflammatory origin, there is no refutation at hand. But as tubercle is not *always* of inflammatory origin, it cannot be essentially an inflammation. It is rather always a degeneration, which has a very habitual relationship to inflammation as a concomitant.

The exudation poured out by inflamed blood into inflamed tissue either fibrillates into induration-matter, or corpusculates into cells, for the most part round and nucleated, which become ultimately organized into tissue, or dissolved and absorbed, or changed into pus cells, according to circumstances; or else, the inflammatory blastema dies rapidly and at once. Tuberculous blastema does none of these after precisely the same type. And when tuberculous exudation is attended with inflammation, we find side by side the normal products of inflamed blood, and the specific products of tuberculous blood. The two are not mutually convertible. It follows, that either that part of the blood-plasma which forms tubercle *cannot* undergo the changes of inflammation; or else, that when some of the blood-plasma is inflamed another portion escapes the process, and is exuded in company with the first, to pursue afterwards a distinct course. In either view, the distinction between tubercle and all the known products of inflammation is obvious. But it may be said, that tuberculous inflammation may be of a specific kind, leading to the specific product of tubercle. Not so: for we find the inflammation which attends tubercle is just like common inflammation in its course, and in its products. If, again, it be urged that part of the process may be common inflammation, and part specific inflammation, proceeding side by side in the same portion of tissue, this seems to be almost a *reductio ad absurdum*, since upon such terms every known disease might be called an inflammation.

Natural Alliances of Pulmonary Tubercle.—Viewed in its twofold aspect, as primarily a degeneration of existing structure, and secondarily an exudation which corpusculates into very lowly-organized cells, tuberculization of the lungs has a generic relation with similar morbid affections of many other organs. And such allied affection of any other viscus, when it happens to coincide with pulmonary tubercle, may be looked upon as equally con-generic, *whether* it stop short at the stage of fatty degeneration, or pass on to that of tubercular formation. Hence the frequent association of phthisis with fatty liver and fatty heart, and its occasional association with the fatty variety of Bright's disease. The occasional occurrence of this affection of the kidney, the more rare occurrence of apoplexy from fatty degeneration of the small bloodvessels of the brain in persons of phthisical habit, but who are not the subjects of developed phthisis; and also the antecedence of general fatness of integument, the fat being less firm than that which indicates mere

* Loc. cit.

excess of good nutrition, in some persons who afterwards become phthisical, may point in the same direction. And several of the not unusual complications of phthisis, as well as some of its customary features, are possibly due to a similar kind of degenerative tendency. Thus, the thinning of the coats of the stomach which occasions the enlargement of that viscus in phthisis, though chiefly owing to simple atrophy of the muscular coats, may be in part also due to fatty degeneration. The hooked and clubbed nails, to fatty degeneration with enlargement of one portion of the bed of the nail. Scrofulous lymphatic glands, to enlargement, fatty degeneration, and accumulation of lymph-cells; or to positive tubercle; or to both, in different instances; inflammation being either coincident or absent. In like manner, the falling off of the hair, so common in females who are phthisical (as well as in many others whose mal-nutrition arises from more innocent causes), may greatly depend on fatty degeneration of the cells secreted by the hair-bulbs unfitting them for development after the existing hair is shed.

Upon these latter points (with the exception of the lymphatic glands), I have no certain knowledge; but reverting to the three instances in which the alliance is clear and sufficiently common—viz., fatty degeneration of heart, liver, and kidney, there is more to be said.

Muscle is not liable to tubercle. We have, therefore, no difficulty in admitting that fatty atrophy of the heart, when it occurs in phthisis, may be directly related to the tuberculous disease, although tubercle itself is not found in the organ. The degree in which the liver and the kidney are prone to become the seat of tubercle is not quite established. All that can be affirmed is, that whilst they are occasionally tuberculized, they are much more liable to be merely affected with fatty degeneration. When so affected in conjunction with tuberculization of the lungs, we need not hesitate to admit a direct relationship between the several organs diseased, in regard to the very essence of the disease. In the fatty form of Bright's disease, the similarity to the primary steps of chronic tuberculization of the lungs is particularly close, keeping in view the normal peculiarities of the two great excretory organs involved.

The lung is a chimney, which requires mere ventilation for carrying out its vaporous excretions. The kidney is a main sewer, always draining and periodically flushed. The internal surface of the lung keeps in all the blood except that portion which evaporates during its passage through the pulmonary capillaries. When, therefore, this surface has lost its natural defence, it does not instantly give exit to complete blood-plasma in excretable quantity. If it does so, there is often a large pouring forth, the blood escapes entire, red globules as well as plasma, and we have hæmoptysis. But in the kidney the internal surface is normally accustomed to let out the watery part of the blood, and so, when bared of epithelium in places, it readily gives passage to a portion of fluid plasma, with or without some red corpuscles, and we have albuminous urine, with or without hæmaturia. In the lung, tubercle follows; as a rule, it does not in the kidney, perhaps for these reasons:—So long as the plasma is periodically washed away with the urine, there is no material to corpusculate left behind; and when checked elimination of urine at length ensues, its consequences generally put an end to life too speedily to permit the question to be tested, not to mention the altered blood-crisis then induced. Moreover, when tubercle

is laid down in the kidney, its seat of election is the cortical parenchyma, and not the interior of the uriniferous tubes. And although the exact relation of tuberculous deposit to the minute structure of the cortex is not clearly ascertained at present,* it is quite possible that the free discharge of albumen from one portion of the gland may oppose the formation of tubercle in the other.

As in chronic phthisis the earliest local process consists in the enlargement and fatty degeneration of epithelial cells, and more or less accumulation of these in the air-vesicles; so also, in some cases in which, had the patient lived, he would probably have shown the customary signs of the fatty variety of morbus Brightii, do we find the uriniferous tubes so choked up with detached granulous and fatty epithelium, that the urine only filtrates through, and carries no albumen with it. Such a condition we shall generally find after death, when, in the last stage of slow phthisis, there is œdema of the lower extremities (irremovable by remedies), without any albumen in the urine, but with a copious sediment of phosphates.

In practice we notice, in respect to this subject, three kinds of case: the one just mentioned; phthisis with albuminous urine; and phthisis arrested, in which the patient dies at some future time of Bright's disease.

Is, then, in conclusion, tuberculization merely an advanced stage of fatty degeneration when this affects certain textures? Certainly not, since fatty degeneration is related to many other morbid conditions besides tubercle, and having little in common with it. But it may be that it is a constant attendant upon tubercle; in which case we have a right to consider tuberculization as fatty degeneration *plus*—something else. If so, all that is known respecting fatty degeneration has a certain reference to the etiology and treatment of phthisis.

Not to occupy space by compiling information easily obtained from the writings of Mr. Bowman, Dr. Quain, Mr. Paget, and Dr. Handfield Jones, I will merely state here that I have succeeded on a large scale in inducing fatty degeneration of various viscera by keeping animals under such conditions as entailed mere inactive discharge of the functions of these viscera.

Neither is tubercle in the lungs the necessary consequence of shedding of the epithelial cells in the air-vesicles, and of subsequent exudation of plasma; for this occurs to some extent in simple pneumonia, and to a much larger extent in the intensely-congested lungs after section or ligation of both *paria vasa* at the same time. Here the deep, forcible, and very slow and infrequent inspirations, and the congestion of the pulmonary bloodvessels, acting as pressure alternating in opposite directions, cause the serum to transude and wash off the epithelium of the air-vesicles. The serum is churned into a spumous froth; hæmorrhage follows in a degree which varies from mere reddening of the foam to pulmonary apoplexy. Inflammation and suppuration may afterwards ensue, but no tubercle. As the animal seldom lives long, how can we know this? If a few days be allowed to intervene between the injury of the two nerves, the animal will sometimes live. Birds will generally survive under any circumstances. Killed some months afterwards, we are able to trace in these

* See, however, an excellent essay by Dr. Duncan, of Colchester, in the Transactions of the Provincial Medical and Surgical Association, vol. xvii. p. 113. 1850.

animals the morbid actions which ensued in the lungs, and we find no trace of tubercle.*

Neither fatty degeneration alone, nor exuviation of epithelium alone, nor mere exudation alone, is enough to constitute tubercle. The exudation must ever be at the first one of tuberculous blood-plasma.

But, in point of fact, as regards the lungs, are these three morbid processes ever conjoined in sequence, unless the blood be tuberculous? Does it ever happen that the epithelium of the air-vesicles degenerates fattily in considerable quantity, and is shed; and that this is succeeded by exudation into the air-vesicles, except in cases of tubercular phthisis? To this question, I am not at present prepared to furnish a positive reply.

(To be continued.)

ART. II.

On the Means of Diagnosing the Internal Diseases of the Eye. By C. BADER, M.D., and BRANSBY ROBERTS, Esq., Resident Medical Officer Royal London Ophthalmic Hospital, Moorfields. August, 1854.

WITH the publication of the following researches we bring before the notice of the profession an improved means of diagnosis in internal diseases of the eye—viz., a simplified ophthalmoscope. The instrument is not new by name; but those already in use are, generally speaking, so complicated and difficult to adjust, that they are but seldom employed.†

Believing that the diseases of the interior of the eye required cultivation, we have jotted down shortly a little advice respecting the management of the mirror, and some portion of the slight experience which we have acquired. It is exclusively to the invariable kindness of the physicians and surgeons of the Royal Ophthalmic Hospital, Moorfields, that the profession is indebted for enabling us to procure, in such a short time, so large a number of cases for examination, and thus fitting us in some way to give assistance to others.

For the different organs in the eye which we are about to examine, we use either a plane or a concave mirror for reflecting the light thrown from a lamp into the eye of the patient. That the eye of the observer may be in the centre of the rays of light thus reflected, a small circular aperture is made in the centre of the glass mirror, either by simply cutting a small round hole in the metallic back to the mirror—scratching off the

* These observations are founded upon experiments made, in 1847, with the assistance of Dr. Small Hall (Cambridge) and Mr. Cotton, on forty-eight animals of different kinds.

† I saw the present simplified mirror first used at the Clinique of Dr. Demarees, at Paris, where it was introduced by Dr. Anagnostakis, of Athens, who has published a dissertation, in which he speaks of the mirror as his own invention; I have felt it my duty, therefore, as a German, to offer some explanation of this. Dr. Anagnostakis speaks in his dissertation, from the book of Prof. Coccius, at Leipzig, about the ophthalmoscope as an *opuscule remarquable*, therefore, he must have known it. I may mention here the words of a letter from Prof. Coccius, which I received a few weeks ago: "In the year 1853 I drew a parallel between the advantages resulting from the use of a slightly concave and perfectly plane mirror, joined with a convex lens (I preferred the latter), and have shown (p. 81 in a book upon the ophthalmoscope, *opuscule remarquable*) that, optically, both forms of mirror are alike, but that the plane mirror, joined with a convex lens, furnishes more advantages in an examination near the eye." The curious may read the dissertation of Dr. Anagnostakis, for the purpose of seeing how much he says concerning that of Prof. Coccius; for my own part, I am quite unable to detect any difference between the concave mirror of Dr. Anagnostakis and that of Prof. Coccius. But Professor Coccius, at another time, will undertake to put Dr. Anagnostakis in mind of the concave mirror which he saw used at Berlin, at the Clinique of Dr. Graefe.—Dr. C. BADER.

silvering, but leaving the glass entire—or boring the aperture through the glass also. (The latter has been generally employed, but Mr. Hawes, of 79, Leadenhall-street, is now making some of the former, in which, also, the diameter of the aperture has been reduced, and which we think will be found of some slight advantage.) Through this aperture the observer looks. The concave mirror—which must be well made, and whose surface must reflect quite equably—is round, has a diameter of about two and a half inches, and a focal distance of about six inches: in its centre, a round aperture of a diameter of two lines,—the whole being fixed to a handle for convenience in using. The diameter, the opening in the centre, &c., are of the same size, both in the plane and the concave mirror. The light reflected by the former being more feeble, is better adapted for examining the crystalline lens, to detect opacity of which, as well as of its capsule, it will be found quite sufficient.

In the examination of all eyes, excepting those of short-sighted people, we were obliged, for the sake of seeing clearly, to use a bi-convex lens of two and a half inch focus, our sight being tolerably natural. Short-sighted people have the advantage of being able to see clearly without the lens, and will only require it for the purpose of concentrating on a smaller surface the objects which, seen through the crystalline lens, appear highly magnified. The usual course we have adopted in the examination of any case is the following:—The patient is seated in a dark room (his pupils having been well dilated by a solution of atropine—gr. j. or gr ij. ad. 3j.); opposite him is seated the examiner—(a music stool is the most convenient seat for the observer, as it can be raised or lowered as required, to suit the height of your patient.) As near as possible to the patient's head is placed a steady-burning lamp, giving a good light, the eye of the examiner, that of his patient, and the flame of the lamp, being on the same level. The observer then takes the mirror and turns the reflecting surface towards the lamp, and endeavours to throw the light upon the patient's eye. The mirror having been taken, and the eye adapted to the central opening (where it must be steadily retained, the head, hand, and mirror being slightly moved, as required, together, either from side to side or slightly round, so as to throw the light fully upon the eye), he must look, having both eyes open, for the round circle of light about as large as the diameter of the mirror: the patient keeping his eye well open, quiet, and fixed; if the left eye is being examined, looking slightly towards the left side of the head of the examiner—the reverse when the right eye is under examination. To return: when the reflected image upon the examined eye is about the same size as the diameter of the mirror—the eye of the examiner and that of the patient, and the flame of the lamp, being on the same level, he looks through the central opening (to make sure of this he shuts for a moment the other eye), and being in the centre of the rays of light, he sees behind the pupil an illuminated red space. Looking into that space from different sides (the patient keeping his eye quite quiet), he will see inwards, and a little downwards, instead of the general red surface, a white, brilliant shining: immediately upon seeing this he brings down the convex lens (which he holds ready in the other hand, just above the examined eye) in front of the eye, so that the light from the mirror may pass through the convex

lens as it enters the eye: and now he sees a most brilliant sight:—the large white surface in the eye, which he previously saw shrinking together, forms a sharply-marked brilliant white circle, of a diameter of about three lines (the entrance of the optic nerve), from whose centre are seen passing a vein and an artery upwards, and a vein and an artery downwards, over the red concavity of the eyeball. This white surface is seen to be surrounded by the beautiful red choroid. Sometimes, before you catch the white surface of the entrance of the optic nerve, upon the red bottom of the eye are seen running darker streaks (the vessels coming from the entrance of the optic nerve), by following which in their backward course, as they become larger—that is, towards the internal and inferior part of the eye—for which purpose the head must be moved in the opposite direction, he will invariably be led to the white surface of the optic nerve.

During the whole examination, the left free eye (supposing the right to be used) must watch the movements of the globe which is being examined, and the reflection of the mirror; which reflection, in all movements with the mirror, must rest upon the eye. Having found the entrance of the optic nerve, and brought the convex lens in front of the eye, if he does not see quite clearly he must carefully endeavour to reach the convenient and proper distance by moving the head, gently and slowly, either forwards or backwards, as may be required, always keeping the mirror close to his own eye. After the examination of a few cases every one is able readily to find for himself the entrance of the optic nerve vessels, &c.

For rendering the examination easier, he may remember the properties of the convex lens—viz., that he will be able, by moving it towards any side, to bring other parts into the field of vision which had been previously hidden; for, supposing the eye to be quiet, we have a field of vision corresponding to the dilatation of the pupil, the objects in that field being immovable: if you now bring the convex lens in front of the eye, the lens is so far from the bottom of the eye that you are unable to see through it the whole surface, limited as it is by the pupil, but only a small part—as, for instance, a portion of a vessel with some surrounding choroid, or half the optic nerve with some choroid. For examining the whole surface, therefore, you are compelled to move the convex lens in a direction opposite to that in which the objects you wish to see are contained. For instance, if he sees only the lower half of the entrance of the optic nerve, and the iris appears to prevent his seeing the other half, by moving the convex lens upwards he sees the image of the optic nerve descend downwards till the whole surface is exposed to view. It must be always borne in mind that the iris limits the field of vision, and therefore, in order to see the whole surface through the convex lens, it (the lens) must either be moved about, or the examiner must move his own head with the mirror, or worst of all, for the patient cannot command his eye sufficiently, he (the patient) must be directed to move his eye slowly in every direction, till the whole has been brought into view. The transparent media of the eye magnify so much that, for overlooking a larger field of vision, for diminishing the size of the objects, and for bringing them nearer together, we are compelled to use the convex lens. As soon as the entrance of the optic nerve is found, you are assured that you have

arrived at the bottom of the eye, having seen through all the transparent media. This spot is the first and the most important that we have to look for, and the examination of other parts should not be prosecuted till everything that belongs to the entrance of the optic nerve has been thoroughly examined. If only a small portion of the crystalline lens remains clear, the entrance of the optic nerve must first be looked for. By observing this white surface, not only do we see the vessels entering and taking their course therefrom, but we see likewise the changes, if any, of and about its surface, as well as in the space in front of it.

The further examination of the eye is very easy. Whenever the entrance of the optic nerve can be readily found by the observer, he can easily learn by his own experience how to go on; the following limited remarks, however, may serve to facilitate the inquiry.

After the examination of the entrance of the optic nerve, we should notice the vessels entering in its centre (always remembering their normal state); then their course over the white surface; afterwards, their relations; and finally, their passage upon, and course over, the red surface, &c. After the vessels, the subjacent retina had better be noticed—its transparency, and its relation to the choroid. Following this, the yellow spot (making the patient look in the centre of the mirror); subsequently, the layer of pigment over the choroid, and the choroid itself; which, after several examinations (for which short-sighted people are the best subjects), being well understood, in its relations to the entrance of the optic nerve, to the vessels and transparent media, we come to the lens, for which, provided there are no muscæ in the vitreous humour (*vide* vitreous humour), we may use the plane or concave mirror, with or without convex lens, and must examine at every distance and from all sides.

The lamp being placed at the side of the patient, allows the eye of the examiner to go as close as possible to the crystalline lens, without losing the light on the eye. The free eye, as said before, should watch the image of the mirror as well as the movements of the globe.

Opacities of the lens are generally stellated in form, and, in consequence of their covering other objects lying deeper than themselves, they are readily seen.

If some time has been spent in strictly following out the course here described, first looking for the entrance of the optic nerve, travelling from behind forwards, and in this way carefully studying as it were the geography of the internal parts of the globe, then the examination of cases where the lens is absent, or the retina detached, or tumours are resting on one or other side of the eyeball, is very easy.

For common examinations the concave mirror is to be selected; but when a very feeble light is required, and you want to examine the anterior parts of the vitreous and the lens, then the plane mirror is to be preferred. If a very strong light is desired, let a tolerably strong convex lens be held between the lamp and the mirror, so that the light may be concentrated upon the mirror. For holding between the mirror and the eye, a good bi-convex lens of two and a half inch focus is all that is required.

Every examination ought to be begun without using the lens.

The following is a short account of the changes which are to be observed in the internal tissues of the globe:

Entrance of the Optic Nerve.—As soon as the light is thrown into the eye from the mirror, the patient looking straight forward, the observer, if he looks a little from the outer side, sees, instead of the red fundus of the globe, a glittering white surface; on approaching this he will find it to be circular, of a brilliant white colour, having a radius of about two lines; in the centre are seen vessels entering arteries and veins, which run upwards and downwards along the concave surface of the globe. This, the entrance of the optic nerve, the fixed point at the bottom of the eye, which must be first seen to make sure that you see through all the media of the eye in front of it, is of considerable pathological importance, not only on its own account, but likewise for the purpose of deciding what belongs to the choroid and what to the retina. In all objects lying behind the lens you must remember that you are looking through that body, and that, consequently, whatever you see is highly magnified. In a later part we shall speak of the vessels coming from the centre of the optic nerve, but at present we will confine our attention to the pathological changes which are most frequently met with on the surface and at its periphery. Any change of or upon the surface, or at the periphery, seems to have a marked influence upon the vision, and up to the present time we were unable to recognise any distinctive character in that portion of the retina which lies to the outer side, and is, as it were, cut off by some pathological change in or about the entrance of the optic nerve, from the other parts of the retina which are not so affected, so as to show that the part of the retina corresponding to the change at the periphery, or on the surface of the entrance of the optic nerve, was distinctly altered in its functions. The normal diameter of the entrance of the optic nerve, which is easily borne in mind, exhibits slight differences in its size at a first glance; but of more than 600 eyes which, up to the present time, have been examined, only one was found in which the surface of the entrance of the optic nerve was remarkably large, in which case the diameter of the circle was about seven lines, the eye perfectly healthy with this exception. The patient, who was a vigorous man, without any known cause had lost all sensibility to light. Where white flakes are seen in the choroid close to the periphery, care must be taken not to look upon this as an enlargement of the surface of the entrance of the optic nerve. In two cases only was the surface of the entrance seen to be diminished. Here the white surface appeared to be drawn backwards, giving it the appearance of a dimple. There are frequently cases in which the entrance of the optic nerve *appears* to be diminished, owing to an infiltrated or detached choroid or retina overhanging it. A little care will remove this source of error.

A brilliant white surface, or a white surface covered with a slight greyish gauze, is the normal appearance of the entrance of the optic nerve, but the limits between normal and diseased parts are so minute, and our knowledge of the degree of organic change necessary to produce impairment of vision so insufficient, that we are obliged to confine ourselves to relating simply what we have seen. We would only remark, however, that in judging respecting an examined case, having compared all the pathological

changes, we should afterwards examine the greater influence of one or the other.

In several cases the white surface of the entrance of the optic nerve was remarkably brilliant, and of a slight greyish hue; where, at the same time, there existed an effusion of serum behind the choroid, which was pushed forwards.

The greyish gauze upon the white surface generally consists of very fine points thickly standing together; these granules of pigment appear diffused through all the layers of the retina. In some cases they are in layers one over the other, in others they form a fine greyish ring upon the white surface; very often they are confined to the entrance of the optic nerve; in other cases they pass over upon the retina; occasionally large brown spots were seen resting upon the boundary of the entrance of the optic nerve and the choroid; while, at the same time, the choroid was seen to be covered with them, so that their situation was with difficulty to be accounted for, unless by an emigration, as it were, from the choroid. In a few cases, but very few, the spots were seen lying over the vessels. White flakes in the choroid are the best indications for judging whether the deposits of pigment are confined to the entrance of the optic nerve, or go over upon the retina.

The same care must be used with regard to the red gauze which is sometimes seen lying over the entrance of the optic nerve. The observer should keep well in mind this reddish gauze, and the layer of pigment which lies over it (or the granules of pigment imbedded in it), modifying its colour and its circular form. He should also mark well the ring of pigment which frequently is situated around the periphery of the optic nerve. For, unless these circumstances are kept continually in mind, the observer will be perpetually thinking the entrance of the optic nerve larger or smaller, in consequence of changes which have taken place around it in the choroid.

On several occasions effusions of blood were seen in the entrance of the optic nerve, either without any bloodvessel about them, or more frequently surrounded by a red gauze, which seemed to be situated below the vessels coming from the midst of the entrance of the optic nerve; the vessels composing the red gauze above-mentioned cannot always be seen, or even the trunks from which they originate, but in some cases very fine branches are observable leaving the vessels at the entrance of the optic nerve, and forming a red gauze or network over its surface; but in these cases two layers are frequently to be seen—a superficial one, formed by fine vessels coming from the entrance of the optic nerve, and a deep one which can only be seen as a gauze, and is generally confined to the surface, whereas the former passes over upon the retina.

The red gauze confined to the entrance of the optic nerve is often found, combined with an anæmic choroid and small vessels, coming from the centre of the optic nerve. The double layer is generally observed to be accompanied by a congested state of the choroid, the vessels of the retina, chiefly the veins, being very numerous. The appearance of the red gauze is sometimes simulated by the choroid hanging over the entrance of the optic nerve, and often exists distinctly, without any change visible in the other blood-carrying tissues of the eye. It was several times seen

in young women who had worked much with the eyes. Few veins were to be seen, but many small arteries coming from the entrance of the optic nerve. The choroid was not much congested. Sometimes the red gauze is the only distinguishing mark between the entrance of the optic nerve and the choroid.

We call the periphery of the optic nerve that circular border of the entrance of the optic nerve, as we see it in the eye, which is surrounded, in the healthy states, by the choroid. This border, or periphery, forms a fixed line between the choroid and entrance of the optic nerve, and is generally surrounded by a slight greyish ring of pigment, composed of fine greyish points, which appears to rest on the choroid. By means of the periphery of the optic nerve, we know what is on the same plane with the choroid, what lies above it in the retina, or is situated over both. In the examination of any pathological change, we first look for this boundary (the periphery), and the state of the vessels running over it; for it is but very seldom indeed altered in position (in two cases it was irregular, and thrown into folds backwards, owing to protrusion of the globe). Effusion of serum behind the choroid or retina detaches and pushes them forwards, causing them to form a wall around the entrance of the optic nerve, which overhangs the periphery, and hides it either partially or entirely.

Whenever the retina or the white surface of the entrance of the optic nerve exhibits an extraordinary brilliancy, partaking of a greenish or bluish glittering, there exists the probability of serous infiltration; and attention must be given to the parts around the periphery of the optic nerve. The periphery being a fixed point, prevents the further extension of the serous effusion, and the infiltrated or detached parts are raised around and hang over it, supposing the choroid is not pushed forwards, but only the retina; the wall hanging over the periphery has a slight reddish appearance, is transparent, and allows the choroid around the entrance of the optic nerve to be seen through it. All tissues over the entrance of the optic nerve appear to have a firmer connexion with it than with other parts; for instance, take the case of vessels coming from the centre of the optic nerve (the characteristic tests of serous infiltration, and protrusion of the parts lying around the periphery). In such an example, the retina can be partially detached, or is overloaded with serum, or both retina and choroid are pushed forwards by serum effused behind them, and so are raised to a higher level than the plane of the optic nerve, or even hang over it; in the former case, the vessels from the optic nerve run as far as the wall, and are then seen to ascend it, and continue their course over the retina; in the other case they run and disappear beneath the wall, again appear a little on one side in the same order, ascend the wall, and continue their course. It is necessary to notice the order in which arteries and veins go beneath the wall, for the purpose of ascertaining that those which are seen ascending are the same as those that went below it.

Up to the present time it was not possible to ascertain the amount of influence which the deposit of pigment in that part of the choroid immediately surrounding the entrance of the optic nerve, exercised upon the functions of the retina. The deposit, to a slight extent, is observed in

what appear to be in other respects healthy eyes, in a greater degree; considerable impairment of vision always accompanies it; continued observation will show whether the reason of the impairment is, that having been the product of inflammation, that part of the retina corresponding to it is injured. In several cases the deposit of pigment was, without doubt, combined with a finished process of inflammation, a cicatrix being visible in the choroid at the periphery of the optic nerve.

The Vessels coming in and going out from the midst of the Entrance of the Optic Nerve.—On the illuminated red fundus of the globe are seen running darker-coloured streaks; by following them backwards they are seen to become larger, and all converge towards the white surface of the entrance of the optic nerve, in the midst of which they disappear. On approaching to a proper distance for observing them distinctly, the streaks are seen to be really veins and arteries, the latter coming from, the former going to, the entrance of the optic nerve. The normal condition seems to be, a vein and artery going upwards, and another going downwards; sometimes the vein and artery have the same entrance, sometimes different entrances. Occasionally each vessel seems to have its own opening, but always the entrance is in the centre of the white surface of the optic nerve. Often the arteries are seen entering to the outer side of the veins, and in their course either crossing the veins, or being crossed by them. The arteries are easily distinguished from the veins by their straighter course, brighter colour, and lesser diameter. In pathological states, where the veins are in greater number, enlarged, and more tortuous, the difference is still more striking. Around the entrance of the vessels in the eye is seen occasionally a little bluish ring or black dimple. Few or no branches are generally given off to the white surface of the optic nerve, but the vessels pass over the surface and its periphery without showing the least deviation in their course, and run along the concavity of the choroid, exhibiting a strong contrast to it. The veins are slightly tortuous, both arteries and veins rarely ramifying; the red surface of the choroid makes it impossible to see the finer branches, and the iris (behind which they disappear) prevents us from following them to their termination. It is very remarkable that in all healthy eyes which have been examined, the arteries and veins have always been seen running upwards and downwards, dividing the concavity of the eyeball into equal halves; and that no visible vessel has ever been seen taking its course over the situation of the yellow spot. The veins and arteries take their way always running near each other; the vessels have transparent walls, and are in close connexion with the retina and hyaloid membrane, because, whenever one is detached, the part of the vessel corresponding to it is detached likewise. The blood in these detached vessels appears to be of a blackish red, and different colour to those which run in part of the membrane still in contact with the choroid.

The pathological conditions of the vessels are alterations in number, diameter, course, and contents; for instance, a vessel taking its course over the situation of the yellow spot would be called abnormal, likewise the tortuosity of the veins, in some places, with or without enlargement; these tortuous veins are seen upon the surface of the entrance of the optic nerve, upon the choroid, but chiefly at its highest concavity, being frequently surrounded by a mass of black and brownish spots.

Sometimes scarcely any arteries are seen entering the eye, in other cases they are very numerous, radiating on all sides, but ordinarily very slender.

In the majority of instances the cause of the tortuosity of the veins did not appear to lie in the eye itself (in one case the pressure upon the vessel behind the eye was quite clear); at the same time, the arteries are very small and anæmic, although sometimes numerous; the tortuosities of the veins are often confined to the entrance of the optic nerve; in some other cases, where the cause of increased number of veins and their enlargement, was in the eye itself, the veins appeared to be carrying very dark blood, were much enlarged and very numerous, converging from all sides towards the entrance of the optic nerve, but they were not tortuous. An augmentation of the number of the veins is not always accompanied by a congested choroid; for sometimes the choroid has been seen to be very anæmic, and yet the veins were enlarged and increased in number; for instance, in serous infiltration of the choroid and retina. The arteries, on the contrary, appeared to be diminished in number, very slender, and very deficient in blood; sometimes no arteries were to be seen at all. Augmentation of the arteries and veins was found to be very common in shortsighted people, as well as in young people who had tried their eyes much by their occupation; in the latter case, the veins were seen to be slightly enlarged, and running a straight course, the arteries generally very thin and numerous. Sometimes a white streak was seen, commencing at the first entrance of the artery, more frequently where the artery began to split into branches, giving the appearance of the blood running along the side of the vessel; whether the vessel is compressed in its centre, and the blood pushed towards the sides, or whether the white streak is a nerve, or is simply the division of the vessel into two finer branches, we are at present quite unable to say. In one case, the vessels coming from the entrance of the optic nerve were seen to form several layers over the red surface; on examining these from the side, they were seen to be separated by some transparent substance. In effusion of blood upon the choroid, the arteries were seen, immediately upon their entrance into the eye, to be enlarged for a short distance; sometimes they appeared to form a rich network over the retina, which contrasted beautifully with the choroid lying below. No certain and true conclusion, in all cases, respecting the state of the vessels in the interior of the eye, can be arrived at from external examination; alterations in the course of the vessels can be best seen over the periphery of the optic nerve, where they are met with most frequently.

Choroid.—The light having passed through the transparent media in front of the retina, passes through that organ, and falls upon a brilliant red surface, which is the first thing which strikes the observer in the examination of the eye; the light reflected from the surface in the transparent media gives this bright red appearance. The choroid (the red surface lining the posterior wall of the vitreous space) consists, judging from the usual appearance in healthy eyes, of bloodvessels lying as close together as possible, and having perfectly transparent walls; the whole being covered by a very tender, pointed, greyish-brown layer of pigment, giving the appearance of an uniform red surface (a comparison of many

healthy eyes of different ages is needed for the purpose of having a correct idea of the normal red of the choroid; likewise, one eye must be compared with the other before you can determine about the degree of congestion, anæmia, &c., present).

Over this red surface are seen running darker red vessels, well contrasted with it; if you follow the course of the vessel backwards, as it becomes larger, it leads to a circular, white, brilliant-looking place, in the centre of which that vessel, with others coming from all sides, disappears (this is the entrance of the optic nerve); and the vessels going out from its midst must first be seen before we can judge of the state of the choroid; as soon as these vessels are seen distinctly, you are certain also to be able to discern the choroid; the periphery of the optic nerve renders it possible to examine accurately the borders of the surrounding choroid; the vessels running over the red surface are easily used as marks, which limit any particular portion for observation, and show what lies upon the choroid, what upon the vessels next the vitreous humour, any inequality of the retina, and enable us to draw a parallel between its pathological changes and those of the choroid.

We are able to examine the choroid a little further than the highest concavity of the vitreous space towards the ciliary processes, without, however, being able to reach them; the most direct light falling upon the choroid about the entrance of the optic nerve, renders that portion always clearer than the parts next the higher concavity of the globe, which ought to be borne in mind in judging about anæmic or congested choroids, &c. Having found the entrance of the optic nerve, we should always examine the place corresponding to the yellow spot, by making the patient look at the aperture in the centre of the mirror.

After the examination of both eyes, and having compared the result with his own experience of the normal condition, he may then look upon it as if it were independent of the layer of pigment placed in front of it; separating in his own mind, as it were, the influence of the pigment upon the red colour; and having thus a clear idea of the mass of blood contained in the choroid, he may compare it (the choroid) with the other pathological changes upon and around it.

The external aspect of the patient does not always enable us to draw any conclusion as to the state of the vessels in the interior; but the knowledge of the condition of the vessels in the eye will often be one of the utmost importance, as a means of diagnosis concerning the state of the vessels throughout the whole organism, as valuable in the treatment of the general as of the local affection.

Our attention must also be directed to the unequal distribution of blood often observed in the choroid (to confound effusion of blood and congestion is almost impossible, because the difference in colour is well marked, and the borders of the effusion well defined). The lighter coloured places may be normal or anæmic; at the same time we may compare the functions of the retina over these points with that at other parts. Sometimes one half of the eye is richer in blood than the other, which is remarkably the case in some squinting people. Some places may be perfectly bloodless, others of a feeble red colour, without the patient complaining of impaired vision. The congested state of the choroid does not always correspond with the exterior of the eye.

The observation of many cases of congestion gives the impression of an active and of a passive congestion; in the former, the choroid is of a bright red hue; the vessels, arteries, and veins upon the retina numerous, but slender, having a straight course, the veins not enlarged. Only a small quantity of pigment is seen upon the choroid, the transparent media in front of which are very brilliant; sometimes in the lens are seen undulating, transverse, and transparent streaks; this form is frequent in young plethoric people; in three cases it was accompanied by enlargement of the thyroid gland, in one by hypertrophy of the left ventricle; in no single case of active congestion were corpuscles seen floating in the vitreous humour.

In the passive form of congestion, the choroid is of a dark red colour, the veins running over it enlarged, numerous, and tortuous; but few arteries, and those of small diameter, visible; the choroid, covered by brownish-red spots of pigment, in some places placed more thickly together than in others. In the vitreous humour are seen floating bodies of the same colour as the pigment; similar deposits are also seen sometimes upon the posterior surface of the capsule of the lens. Till the present time, but little has been known, comparatively, of the state of the choroid, in inflammations of the iris, sclerotica, &c. A little exercise in the management of the mirror makes a glance almost sufficient to determine the condition of the choroid. How far the feeble light of a candle, concentrated upon the retina, augments the inflammation, it has been hitherto impossible to know.

The degree of Congestion is not sufficient to account for the Impairment of Vision.—In congested eyes, the observer must notice the mass of pigment lying upon the choroid, its distribution and form; likewise the number, direction, and size of the vessels coming from the centre of the optic nerve; also the vitreous humour, lens, &c.; for the purpose of knowing the extent of the congestion, as well as for estimating how far the other tissues of the globe are affected. In some cases of active congestion, on approaching the eye so as to see not quite clearly the form of the vessels, a circular shadow will be seen upon the convexity of the lens, surrounded by a luminous ring, behind which appears the illuminated vitreous space, of a brighter colour, however, behind the luminous ring, than behind the greater convexity of the lens; whether the lens is pushed slightly forwards, or what is the cause of the shadow, we have not been able to determine: What confidence can we have in the means employed against congestion, when we see the vessels so distinctly?

The external appearance of the eye frequently agrees with the anæmic state of the choroid, but not always. The diagnosis of these conditions is easy so soon as you have a true and accurate idea of the normal red colour of the choroid; and these concomitant organic changes in the eye being strongly marked, can easily be distinguished from any other disease. The eye of the observer, accustomed to a red surface of a certain shade, sees instead of it (chiefly where the most light falls upon it, as about the entrance of the optic nerve) a reddish white or dirty white surface, covered with greyish, black, or brownish flakes of pigment, over which surface are generally seen running very slender veins, and, only one or two arteries coming from the centre of the optic nerve. Not seldom the

vitreous is fluid, and muscæ are seen floating in it; in very many cases, cataract is seen commencing, and of it this anæmic state of the choroid frequently seems to be the forerunner. Cases of anæmia were most frequently seen between the ages of thirty-eight and fifty; many complained of a cloud before the sight, and of soon feeling fatigued upon using the eyes.

In all diseases of the eye, it is very interesting to observe the state of the layer of pigment upon the choroid; what is the normal condition is difficult to say; in about thirty healthy eyes which have been examined, it appeared as a thin, greyish-brown gauze, placed over the red in the form of points. The accumulation of pigment in masses seems to be one of the first visible symptoms of a diseased condition of the interior of the eye; the pigment was never seen in its normal state in any cases which have been examined where the other tissues of the globe were diseased. It was always accumulated in largest quantity where the choroid surrounded other tissues or pathological products in its own plane; as, for instance, at the periphery of the optic nerve, around bloodless places of the choroid, towards the ciliary processes, &c. In six cases of detached retina, which have been examined, no pigment at all was seen upon the choroid, which was of a feeble red colour; yet in these cases the vitreous humour was full of detached portions of pigment. After iritis of a severe form, rich accumulations of pigment have been always seen either deposited upon the capsule of the lens, on the choroid, or swimming in the vitreous humour. Very remarkable and almost characteristic of anæmia, is the mass of pigment which may be seen seated upon the choroid, around the entrance of the optic nerve, as a fine mass of points, of varying colour—greyish, greyish-brown, brown, and black—and, toward the highest concavity of the globe, as large surfaces collected together, forming small islands of pigment upon the anæmic choroid. Sometimes below the choroid, carefully examining between the masses of pigment, are seen bright red streaks, which uniting, form larger trunks, and take their course towards the entrance of the optic nerve. Why the pigment is seen better in one case than another?—why in some places it is accumulated together in large masses?—how it arrives in front of the retina in the vitreous space?—whether the so-called muscæ always make their way from the choroid into the vitreous space, & sometimes originate in the vitreous itself?—all these questions are capable of being resolved, we trust, by the use of the mirror.

No comparison can at present be founded between the amount of pigment and the degree of impairment of vision; we can only say, that the pigment is a valuable symptom which, united and compared with others, renders us capable of knowing and distinguishing different diseases.

The presence of quite white surfaces upon the choroid is curious. Hitherto we have only observed them in shortsighted people, where either in or behind the retina was a white layer, which prevented the light from arriving at the choroid, or, what is more probable, the vessels of the choroid itself at that place were obliterated or bloodless; in all cases these surfaces were close to the entrance of the optic nerve; sometimes a few flakes of pigment were situated over its surface; their border was surrounded by a thin line of pigment, and stretching across them was

seen a vessel coming from the entrance of the optic nerve. It is impossible to confound these places with spots in an anæmic choroid poorly supplied with blood, because their borders are distinctly marked by the surrounding choroid, which has a good red colour, and no other symptoms indicative of an anæmic condition can be detected. It perhaps might be mistaken for such a case as we once examined, in which, the radial and temporal arteries were rigid and tortuous; in the congested choroid of this patient were seen quite white, irregular, sharply-marked spaces, in which were scattered about little diffused effusions of blood. These small apoplexies may be sometimes seen, after blows upon the eye, or an attack of apoplexy of the brain, as sharply-marked dark-red flakes, as a mass of points composed of blood, or oval patches, besides the vessels, with or without augmentation of the vessels of the retina.

At the situation of the yellow spot (that is, at the point which is opposite the mirror, when the patient looks at the aperture in the centre of it), in the normal as well as in most diseased eyes which we have at present examined, there is an uniform red space, covered by a thin greyish layer of pigment, over which not any of the vessels coming from the entrance of the optic nerve were seen running; in the few cases in which it was seen to be changed, the vision was considerably impaired; in one case a reddish surface existed, which was surrounded by a bluish-black ring; the other eye was healthy; this patient could only distinguish day from night. In another case, it (the place of the yellow spot) was occupied by an irregular brownish spot, as large as a pin's head, in the centre of this spot was a black point; the other eye healthy; on holding the palm of the hand before the patient, he could only distinguish the tops of the fingers, and of many other objects nothing but the outline.

The vitreous appears behind the iris as a perfectly transparent, quiet mass, placed in front of the choroid which surrounds it. Whether the vitreous in its centre or in its whole extent is fluid in the normal state, has not at present been determined; that it is fluid in a great number of cases of disease, is certain. Movement in the normal vitreous has never hitherto been proved; but when the eye is moved and looks upwards, the vitreous not only is moved passively with it, but after the globe has come to rest, owing to the shock in the first instance, it still remains in motion. We are enabled to see best through that part of the vitreous which lies between the greatest convexity of the lens and the entrance of the optic nerve. On looking from the side into the eye, for the purpose of examining the parts which lie toward the ciliary processes, in a healthy eye we can see nothing but a red glittering. The structure of the lens is not the only reason for this, for in cases in which the lens was quite transparent, and the vitreous humour fluid, we were enabled to follow vessels from their commencement at the entrance of the optic nerve till they disappeared behind the iris, or rather, till we could follow them no further, owing to the free border of the iris. The uniform transparency of the vitreous humour enables us to see the transparency of the retina very clearly. For measuring the distance of objects in the vitreous humour, and judging what lies near the retina, what near the lens, what is placed above and below, what is fixed, and what moves forwards, or backwards,

we must employ the vessels which come from the entrance of the optic nerve and run over the retina in front of the red surface, taking for granted that the latter is not detached, these vessels are immovable, and being clearly seen, form marks by which we learn what is behind them or in the space in front. When the eye moves, they move with it, but immediately the globe comes to rest they become stationary, and so indicate the slightest further motion of any body in the vitreous space. Another means of measuring the distance is the periphery of the optic nerve; still another are the flakes of pigment sometimes seen on the posterior wall of the capsule of the lens, or even streaks in the lens itself, which have portions of lens, sometimes quite transparent, between them, and are, for the anterior part of the vitreous humour, very accurate marks for estimating the size of bodies, their movements, &c. Another means is the different distances at which we use the plane or concave mirror, with or without convex lens; the nearer you approach the vitreous humour, the more superficial must be the object you examine, and the better you are able to distinguish its colour; so that any small body which at eight inches' distance seemed to be black, will at two inches' distance prove to be white. The nearer you approach the vitreous humour, more especially when it is partially obscured, so much the more do the posterior parts serve to stop the light, and it becomes concentrated upon the smaller objects in the anterior portion of the vitreous space. The more light there is behind small objects in the vitreous humour, the darker they appear; but for this they need a certain circumference, for, under a strong illumination, considerable masses suspended in the humour, in the form of points, become quite invisible; and the best plan under such circumstances is to use a plane mirror, which casts a feeble reflection into the eye. Experience is the best instructor for each, as to when the convex lens ought to be used: it would be extending this paper to a great length and little profit if we were to discuss all the cases in which it might be used—it had better be tried in every case. The most frequent pathological conditions of the vitreous met with are its fluidity, in different degrees, and its being obscured by the so-called muscæ, or through effusions into it. Fluidity without any substance floating in it, we have never as yet met with; the degrees of fluidity are various and well-marked, and can be best judged of by the facility with which the bodies move about; they are seen sometimes flying up only to a certain height, and falling back again; often these movements are confined to the space which corresponds to the posterior convexity of the lens; often these bodies form greyish clouds which, in consequence of the shock given to the vitreous by any movement of the eye, take on a rotatory movement, which is confined to the circumference of the lens; often the bodies are so free that they may be seen moving in all directions—disappearing behind the iris—appearing again—never being quiet. The eye which is not employed in looking through the aperture in the mirror, should always be used in watching the movements of the examined globe, for the purpose of comparing the extent of the external movement with that of the bodies in the eye, for measuring the degree of fluidity, &c. Care should also be taken to fix one of the noticed stationary marks at the periphery of the vitreous, to judge concerning the movements in it. Supposing only a small portion of the

lens to be transparent, even then the vitreous will not escape a careful observation.

The cases of fluid vitreous which have been at present examined were in persons of middle and old age, whose attention had been in the first instance drawn to their eyes by the appearance of muscæ; in some cases the disease was distinctly referred to some antecedent syphilitic affection of the eye, in other cases the subjects of the malady were very strong and healthy-looking; in some, cataract existed in the other eye, the opacity being inconsiderable, the vitreous behind perfectly transparent, no muscæ, and the vision comparatively much more interfered with in the other eye, in which the vitreous was fluid and full of long black flakes; it is very seldom that the same degree of fluidity is observed in both; the worst eye is often the one which was affected last (as the patients say).

What is the influence which the different degrees of fluidity exercise upon the vision, is not easy to be determined, because generally the changes of nutrition and their derangements in the eye are so considerable, that no separation is possible.

The questions—What is the cause of the fluidity of the vitreous humour? where it begins? how it ends? what is the cause of the muscæ, and how do they become seated in the vitreous?—can only be answered by long and repeated observation. The determination of the colour of the muscæ is valuable, inasmuch as it enables us to compare those floating in the vitreous with the bodies observed on the choroid. After long-continued inflammation they are found to be black; sometimes, also, they are seen deposited upon the posterior capsule.

Numerous small muscæ appear to interfere more with vision than the larger flakes which hung together and are less numerous, and the movements of the latter are generally very limited—simply from one side to the other. Black muscæ, of the colour of the pigment deposited upon the choroid, do not seem to affect the sight so much as the greyish ones (due regard to other changes in the eye being taken into consideration). These black bodies seem so analogous, in many respects, to the pigment over the choroid, as to at once give rise to the idea that an emigration takes place in some way from the choroid to the vitreous. We are further led to think this by the obliteration of spots of pigment at the same time behind and over the vessels of the retina, so that one layer seems more advanced than the other: for this to be carried out we must suppose an atrophy of portions of the retina. If some of the muscæ are followed in their movements as they fall back behind the iris, they are seen either to sink and disappear in a troubled reddish and brilliant space, or else, after falling to a certain depth, remaining fixed in the humour. Besides these sinking muscæ, a fine greyish or black mass of points is often seen suspended through the whole vitreous; these appeared in some cases to be like the *débris* of larger bodies, in others quite uniform. The plane mirror should always be used in these cases, for sometimes the vitreous examined with the concave mirror appeared to be quite clear, whereas, upon employing the more feeble light of the plane mirror (placed very near the eye), the whole vitreous was seen to be permeated by a mass of very fine greyish points.

A cataract must be very dense not to allow the red space behind it to

be examined. If, after directing the patient to move his eye quickly from side to side, you tell him to look straightforward, and throw the light through one of the less opaque streaks in the lens, when the vitreous is fluid and bodies are floating in it, through the lens, feebly illuminated, small shadows (as it were) are seen flying about.

In examining effusions of blood into the vitreous, its colour must be noticed, for knowing how long it has been effused; its form, for judging concerning its diminution; and the surrounding parts must be searched for exudations about them, or for greyish membranes extending into the vitreous chamber. We believe that many cases of capsular cataract which have been torn through, and in which a good black opening has been formed—the patient not being much improved by the operation—if the eye were examined, many other opaque portions of membrane might have been found stretching through the vitreous in all directions.

An anæmic condition of the eye is, more frequently than any other organic change, found concomitant with fluidity of the vitreous.

Without observing any muscæ in the eye, without being able to detect any mass of points, the vitreous sometimes appears so obscured, that only the outlines of vessels can be seen, and the entrance of the optic nerve as a feeble white glimmer. This dulness has a dirty-brownish aspect; and in all these cases, the exterior of the eye has exhibited marks of chronic degeneration of all the internal tissues, the pupil has been dilated, irregular, and immovable.

The influence of the vitreous and other transparent media of the globe upon vision, we shall leave till further experience has enabled us to speak more practically than theoretically.

Excepting in those cases where the retina and hyaloid, having been detached from the choroid, are floating loose in the vitreous space, it is very difficult to say anything certain respecting the pathological state of the retina, being quite transparent; and we have never been able to prove, with certainty to our own mind, that either spots existed upon it, that it was atrophied, or not perfectly transparent; for when, between a certain point of a vessel coming from the entrance of the optic nerve and the choroid, a black spot or effusion of blood is seen, the retina may be pushed forwards in the transparent vitreous, without being in any way changed. The difficulties of examining perfectly transparent media can only be appreciated by those who have seen the distribution of the light in the vitreous space behind the lens.

How the pathological changes of the other tissues—as of the pigment upon the choroid, congestion of the vessels distributed over the retina—act as disturbing influences upon the latter, is at present impossible to say. It cannot be expected, in the present state of our knowledge, for us to show why one part of the retina is sensible in this way, another portion in a different manner—what may be the influence of this or that change upon its functions.

An œdema of the conjunctiva, with slight congestion of its vessels, has a yellowish-bright aspect; the same impression is conveyed to the eye when the choroid is congested, and the retina appears to be infiltrated with serum. When changes are seen at the situation of the yellow spot, which considerably impair the vision of the patient, we must not forget

(although that part of the retina which is opposite the highest convexity of the lens is impaired, and, therefore, the patient sees badly) to examine other parts of the retina also for the purpose of testing its sensibility.

The cases which have been hitherto examined, have led us to the conviction, that the mirror, to a certain degree, like the stethoscope, will enable us to determine the amount of impairment of vision in the patient's eye, without its being necessary to ask what he complains of; whether he is short or long sighted; whether he sees better with this or that portion of the retina, &c. The congestion and anemia of the choroid, so frequently met with, always exhibit a train of symptoms peculiar to them, so that it is to be hoped that, by persevering study of the organic changes, some light will be at length thrown on the functions of its different parts. The most interesting disease, in an optical and anatomical respect, is the detachment of the retina and hyaloid with the vessels coming from the entrance of the optic nerve. In the small number of cases which have been yet seen, a large portion of, or the whole, retina appeared to be swimming in the fluid vitreous. The mode of examining these cases, and what is to be seen, is more given in detail in the history of each patient; but it may be remarked here, that when the whole retina appeared to be detached, the patient always retained a slight perception of light. These cases ought to be examined in every way, and at every distance; they are, optically, the most instructive. The red surface of the choroid has lost the brilliancy which it received from the closely superimposed transparent retina, and is of a feeble red colour, upon which the pigment is deposited, and over which the vessels are seen running. The entrance of the optic nerve appears as a small surface of the size of a pin-head, occupied by the vessels coming out from its centre (which appear to be chiefly veins) hanging down in the vitreous, and running like ribs over the whitish-grey membrane. The vitreous humour, often obscured by a nearly invisible granular mass of points—sometimes full of mæcæ of different colours; in most cases the lens was quite transparent, and allowed the observer to trace vessels as far forwards in the space behind as was compatible with the presence of the iris. Four of these cases occurred in seamen who had made voyages to tropical climates, and were between the ages of forty and sixty.

Lens.—The examination of the lens with the mirror possesses a double interest:—First, from the certainty of the existence of changes in it which are not to be seen with the naked eye; and, secondly, the possibility of being able to examine, in many cases, the parts behind the opaque lens. Changes in the lens which can be seen with the naked eye are as well seen with it as with the mirror, and before you proceed to your examination with the mirror, the cornea must be carefully observed, that no opacity exists in it; for if such should be the case, the determination of the state of the lens becomes very difficult, not to mention the great likelihood of mistaking an opacity of the cornea for that in the lens. Striæ and slight opacities of the lens are easily known. Supposing these to be present, and you look for the entrance of the optic nerve, it will be found that the eye is prevented from seeing at once the whole surface by some dark mark. To know whether this mark is seated in the lens,

vitreous humour, or still further backwards, the reader must refer to what has been said respecting the movements of muscæ in the vitreous humour. Any opacity in or upon the lens has a movement corresponding to that of the whole globe, and no more, at the same time covering a portion of the background in proportion to the size of the opacity; if you, however, look from the side into the eye, the part previously covered is at once seen.

In examining the lens, the plane and the concave mirror, with or without convex lens, should be employed; and it should be tried at every distance, as well from the front as from the side. Very often the different direction of the rays of light is the only apparent reason for seeing or not seeing. For example: the lens, when looking from the front, may appear perfectly transparent, but on looking at it from the side it is seen to be traversed by light undulating lines, which detain the light in its passage through in a straight line, without, however, producing any opacity of the substance of the lens. These undulating lines are most frequently seen in eyes which are actively congested; sometimes the mass of light exercises a vast influence upon the appearance or non-appearance of opacities.

In one case the changes in the lens, which were tolerably advanced, were only to be seen with the plane mirror. There was not the slightest opacity of the lens, but its transparent elements were so disturbed amongst themselves, that, from whatever side it was looked at, small glittering surfaces were seen, which, although quite transparent, reflected the light differently from the surrounding substance. In another case, this derangement of the fibres of the lens had a curious effect: the patient, having both eyes open, found his vision much impaired, and could only read large print; upon shutting the right eye, he can see with the left the smallest type. In both lenses, bluish streaks, of equal number and development, were seen running from the periphery towards the centre; in the right eye the transparent elements of the lens lying between these streaks were as much deranged in their equality as if they had been stirred about.

Sometimes flakes of pigment are deposited upon the posterior capsule, from which the patients say that they are floating about in the eye.

Anæmia of the choroid is the most frequent disease seen at the same time with opacity of the lens: the relation between them—which the cause and which the effect—in what manner the degree of one corresponds with that of the other—is easy to be decided. The amount of vision does not only depend upon the degree of opacity of the lens, but likewise upon whether the choroid is well supplied with blood or not, for the more blood there is in the choroid (taking it from the anæmic state upwards to its normal condition), the better the sight.

The brilliant surface given to the choroid by the transparent retina seems to play an important part in the formation of cataract; and it would be interesting, in a certain number of cases, to examine the condition of the choroid both before and after the extraction of the lens; for although the removing of the opaque lens is the first indication for the procuring of improved vision, yet the congestion produced in the eye by the operation, and the irritation caused by the light which now falls

without obstacle upon the retina (for a simple examination with the mirror causes a free injection of the vessels), are certainly subjects of some moment, which deserve to be borne in mind. The observation of the influence which the choroid has upon the vision, has caused some doubts to arise concerning the propriety of the rules which are generally given respecting waiting in cases of extraction.

The following are some cases in which it was neither possible with the naked eye, nor with the aid of a convex lens, for throwing more light in the eye, to make out any alteration in the transparent media of the lens:

1. On looking from the front, nothing abnormal to be seen in the lens; at ten inches distance, looking from the side into the eye, undulating lines were seen in the upper part of the lens.

2. It was only possible, with the plane mirror, to see some light striae going from the periphery towards the centre of the lens.

3. From the centre of the lens, towards the periphery, are going six greyish-white streaks, between which the substance of the lens was quite transparent.

4. The patient having received a blow on the eye:—looking from the front, all quite clear; from the side, cracks going from above downwards.

5. The whole posterior wall of the lens full of little brownish points, rendering it difficult to see behind it: when the eye moves, the whole mass of points moves at the same time, no one point approaching the other—the patient always sees something floating before the eye.

6. The patient received a blow from a stone thrown upon the eye:—no inflammation existing at present. From the front, the lens looks quite transparent; examining from the side, the light is reflected by a light crack in the lens, which runs from the upper periphery towards the centre.

A case of conical cornea having been examined, no abnormal change of the deeper tissues could be detected; but no objects lying behind the cornea, upon examination, appeared in the same plane. All lines upon its surface were distorted, and it was only by looking through the side of the cone that the state of the deeper tissues could be determined. The light falling upon the summit of the cone gave it the appearance of a glittering point, surrounded by a dark cloud.

Care must be taken, previous to examining the cornea with the mirror, to see that its surface is quite even and smooth; if not, inequalities upon it may be taken for undulating lines in the lens.

The advantages which have been already derived from the use of the mirror in practice are, we believe, the following:

1st. The possibility of detecting the slightest impediment to the passage of light through the lens.

2nd. Of seeing the reason of the frequent unsatisfactory result of the tearing through of membranes occluding the pupil.

3rd. The advantage of being able, in many cases, to see (through the cataractous lens) the state of the parts behind it,—as the vitreous humour, whether healthy or normal; retina, whether detached, &c.

4th. In the uncertain indications of the external symptoms, we have

the certainty of not mistaking an anæmic for a congested condition of the internal tissues.

5th. The advantage of not treating a patient, who is amaurotic from a detached state of the retina, with too energetic medicines.

ART. III.

On the Peculiarities in Figure, the Disfigurations, and the Customs of the New Zealanders; with Remarks on their Diseases, and on their Modes of Treatment. By ARTHUR S. THOMSON, M.D., Surgeon of the 58th Regiment of Foot.

(Concluded from No. 28, p. 470.)

Wounds and Injuries.—It was to be expected that wounds and injuries would be less frequent among the New Zealanders than the English, because there is no machinery, no building of high houses, no personal combats with fists, and no intemperance among them. The wounds of the New Zealanders heal with great rapidity. I have seen the most severe injuries do well. The famous chief, Hongi, the Napoleon of New Zealand, had a musket wound through the chest and lungs, and lived for several years with a hole in his back, through which he could produce a whistling sound by the expulsion of the air, for the entertainment of his friends when in gay humour. This wound was ultimately the cause of his death.

Diseases of the Eyes.—This class of diseases is frequent. The most frequent form is conjunctivitis and scrofulous ophthalmia, which affections terminate often in partial or complete opacity.

Diseases of the Skin are much more frequent among the New Zealanders than the English. The principal form is psoriasis and scabies, modified by neglect into a most inveterate disease. Tinea capitis is frequent, and also herpes circinatus—ringworm. I have not seen nor heard of a case of herpes zoster.

There are hot springs in the interior, where the natives resort for the cure of cutaneous diseases, the beneficial effects of which may either arise from the sulphur they contain, or the cleanliness they produce.

Scrofula.—Under this term is included, almost entirely, cases of supuration, or swelling of the glands of the neck, king's evil, and some cases of curvature of the spine. There are many other diseases which, with great propriety, might be included under this head; but, without them, it will be seen how painfully prevalent this obscure constitutional disease is among the aborigines of New Zealand.

In some districts, 20 per cent. of the adult natives, taken indiscriminately, are found with the cicatrices of sores on the neck; in other districts, 10 per cent. These are not loose assertions, but numbers obtained from actual enumeration. All, however, who are scrofulous, do not bear on their bodies this outward mark, for "the seeds of disease are so hidden in their constitution, that even the healthiest men, to all outward appearance, are often the first to be taken."*

* Bishop Selwyn's Visitation Tour in 1848, published in *The Church in the Colonies*, No. 20.

No medical man can look at a number of New Zealanders without observing, strongly marked, that peculiar physical development which characterizes the scrofulous diathesis. It is the bane of the whole race, and the remote and predisposing cause of almost all the sickness and mortality which occurs among them. In early childhood we see it producing wasting of the body, fevers, and bowel complaints. After puberty, the swollen and suppurating glands in the neck point out that the disease still lurks in the system; from this period of life until death, exposure to the vicissitudes of the weather, poor food, and a variety of other causes, generate or excite scrofulous disease of the lungs.

The internal use of iodine and cod-liver oil has lately been found by Dr. Davies, the Colonial Surgeon at Auckland, to be a very efficacious remedy. Psoas and lumbar abscesses have been known to prove fatal, and a good many New Zealanders are seen with curvature of the spine.

The causes of this great prevalence of scrofula among them are—breathing an impure air in their sleeping houses, indolent habits, insufficient clothing, bad food, intermarriages with near and scrofulous relations, and change from a tropical to a temperate climate.

To enter minutely into all the above causes of scrofula, is foreign to the object of this paper; but I may observe, that the sleeping houses of the natives are low in the roof, small, and often below the surface of the ground, with a small door and a small window, both of which are shut at night; and for ten hours of the twenty-four the men, women, and children respire an atmosphere which is as unwholesome as the most crowded and worst ventilated houses in some of the poor parts in London. In former days, the constant dread of war, and war itself—the uncertainty and difficulty of obtaining food, previous to the introduction of the fruitful European potatoes, made them exert themselves in fishing, killing birds, rats, gathering seeds, &c.; but now there is little dread of war, and a few days' labour is all that is required to plant potatoes sufficient for the year, and to collect a few pigs, and sell them, so as to obtain blankets and tobacco. The rest of the year is spent in smoking, talking, reading, playing draughts, the indolent occupation of cleaning flax, and sleeping. There are a few persons in some tribes actively engaged in commerce, and in cultivating wheat, but these are the exceptions to the general rule. The clothing of a New Zealander is not sufficient to protect him from the weather. It is a loose gown of common calico (a round-about), buttoned or pinned round the neck, and a rug or blanket overall; the children are similarly clad, and you often see a mother and her infant crouching under a scanty and threadbare blanket. In former days, the quantity of fish eaten was much greater than at present. Potatoes are now more easily got, and are more easily cooked; besides, they have a way of preparing potatoes and maize for food which is highly injurious. It is by making them putrid before they eat them, by steeping them in water. This is evidently a modern evil, the injurious effects of which, I am of opinion, are very great. Intermarriages with near and scrofulous relations must also cause and aggravate scrofula: this is often difficult and impossible to prevent, and the New Zealanders have no inborn dread to incest. I know an instance of a man who has two wives: one of them is his grown-up daughter, and she is the mother of three children to him. The

last cause of scrofula among them is their migration from a tropical to a temperate climate. The New Zealand race keep an exact enumeration of the generations which have passed away since their arrival in New Zealand, and, according to this, it is probable they arrived in New Zealand from the tropics about six or seven hundred years ago. In the tropics, the climate has an average temperature of 84° Fahr.; the country they now live in has a mean temperature of 60° Fahr. There are not many instances of the aborigines of the tropics migrating to the temperate zone in large bodies, but in every instance which has occurred the migration has proved unfortunate.

There may be objections to this last cause of scrofula, and I admit it is difficult to comprehend how a race would increase for several hundred years in an uncongenial climate, and then suddenly decrease; but, from a careful examination of the question, I am clearly of opinion that change of climate must be considered as one of the exciting causes of scrofula, for the New Zealanders flourish best in New Zealand where the climate is warmest, and worst where the climate is coldest.

All other Diseases.—The number of causes included under this head are much more numerous among the English than the aborigines of New Zealand.

During the year 1851, in different parts of the North Island, mumps (*cyuanche parotidæa*) prevailed as an epidemic, and the metastasis to the testicle was several times observed; a few cases of diseases of the heart are recorded, and from the prevalence of rheumatism I am surprised they are not more frequent; hydrocele now and then occurs; otitis and deafness are met with, and are most frequently produced by want of cleanliness. Eight cases of erysipelas have been treated, five of them occurred at Kororariker, in the Bay of Islands, in 1838, when that place was a scene of intemperance; and it is probable this habit may have had some influence in producing the attacks, as I never saw a case of erysipelas among them in Auckland.

The New Zealanders are not intemperate, they will drink spirits if it is given to them, and they like the excitement which it produces; but a New Zealander with a shilling in his pocket will rather purchase a loaf than a glass of spirits. Scurvy is a rare disease. Dracunculus, or Guinea worm, is unknown. Elephantiasis Arabica (elephant leg), which is very common among the Polynesians in the tropical islands in the Pacific, is almost, I was going to say entirely, unknown in New Zealand; but I recollect seeing a native in the interior of the north island with modified elephantiasis of the left leg. Inguinal hernia occurs, and proves occasionally fatal; I saw a strong young man die from strangulated hernia. Double inguinal or labial hernia has been seen in a female; and umbilical hernia is common among children, in consequence of the custom of cutting the cord too short, with the shell which they use for that purpose; it generally disappears after a few years. I have heard no bad result from it.

Cancer.—From very careful inquiries, I have not heard of a native woman dying from cancer or carcinoma of the breast. If the disease should occur, it must be extremely rare indeed. This remarkable peculiarity deserves particular attention; but the absence of the disease from among the New Zealanders is another link in the chain of evidence, that

cancer is a disease of civilization. Sir Astley Cooper recommends a diet of animal food as a means of cure in cancer; but the absence of the disease among a people who live almost entirely on vegetables, would indicate a different opinion. As cancer is said to be rare among the inhabitants of Egypt and Algeria, a migration to these countries has been recommended to those suffering under the malady. The same may be said of New Zealand. But I have already said that, although the aborigines do not suffer from it, yet European women have had the disease commence in the country; and there are no European women sufficiently old, who have been born and brought up in New Zealand, so as to ascertain whether they are liable to the disease or not.

Stone in the Bladder.—No case of this disease has been seen, nor have I heard of one. As Professor Cooper has calculated that one case occurs annually in England for every hundred thousand souls, we might naturally expect, out of a population supposed to be nearly one hundred thousand, that ten years' constant intercourse would have brought one instance of stone under the notice of some of the Europeans living in the country, had the disease existed among the New Zealanders. The vegetable diet, and the large quantity of pure water which the New Zealanders drink, tend to produce urine of a low specific gravity, and comparatively free from uric acid.

Bronchocele.—I heard of one supposed case of this disease, but on inquiry found it to be scrofulous tumour. There is in the North Island of New Zealand, in which almost all the native population live, a large quantity of magnesian limestone, which cannot fail to impregnate the water passing through it; but I have visited villages in this part of the country, but never saw a case of bronchocele. This circumstance furnishes no argument against the theory of the magnesian origin of bronchocele, because the aborigines rarely use well water, and they are very particular that the river water they use is clear and tasteless. If, then, there is one thing more than another the New Zealanders are particular about, it is the purity of the water they drink. Thus, well water is rarely used, or a stream which runs through a wood, where decayed trees are likely to be found.

Hydrophobia has not been seen either among dogs, Europeans, or natives. The absence of the disease in New Zealand is a link in the chain of evidence, that the malady has its origin in specific contagion. There are many dogs in the country, and some of them are very badly fed animals.

Tetanus.—I have not seen nor heard of either an idiopathic or symptomatic case of tetanus among the aborigines.

Diseases of Infancy.—A large number of children die under three years of age. The poor diet on which the mother lives produces thin and watery milk; sometimes the milk is scanty, and food is given to the child which it cannot digest. This produces bowel complaints and fever. Neglected catarrhal complaints from insufficient clothing often produce death. Dentition is occasionally accompanied with irritation and convulsions, the last complaint is not so common as among European children; croup proves fatal; worms are very frequent. *Tabes mesenterica* occurs, but the poor food infants have given to them lays the foundation of a delicate sickly life and a premature old age.

Diseases of the Organs of Menstruation.—As there are a large number of women in New Zealand barren, it may be supposed the menstrual discharge is often irregular or diseased. I have inquired into this subject, but it is a difficult question to arrive at the truth of, because the native women rarely consult Europeans about this class of diseases; from the inquiries I have made, it appears the menses begin to flow at thirteen, fourteen, fifteen, or sixteen. I have heard it stated they begin at ten years of age, but this is not usual. I never saw a New Zealand girl have a child who in appearance was not at least fifteen years of age. I have seen European mothers younger. Sexual intercourse takes place often before the menses appear. The menses are sometimes very irregular, one or two months often passing without a flow; women occasionally have headache for a day or two before the usual period of the menses, but no uneasiness is felt after this. I have heard of some instances of menorrhagia; profuse menstruation occurs. From all inquiries, I am of opinion that the New Zealand women are subject to the same irregularities as women in England: but these irregularities are perhaps not so common, nor do they appear to have so much influence on the constitution. This opinion is founded, however, on no statistical data. Hysteria and chlorosis, the two maladies most influenced by the menses, are almost unknown. I have heard of a half-caste girl who had chlorosis. The sterility which the New Zealand women suffer from, is perhaps caused by too early and promiscuous sexual intercourse. I have been told that women have a way of rendering themselves barren by injuring the womb, but as my informant, one of the clergy of the Church Missionary Society, said it was never attended with loss of life, I am inclined to doubt it. Count Stretzke, in his 'Physical History of New South Wales,' published in 1845, states, that when once a female of the aboriginal tribes of Australia has borne a child to a European or white, she ceases entirely to reproduce with males of her own race. I doubt the truth of this among the aborigines of Australia, and can deny its accuracy regarding the New Zealand women, for I know several instances of New Zealand women having children with men of their own race, after having had children by Europeans.

Parturition is not attended with the same dangers as among Europeans. Many missionaries and medical men never heard of a woman dying in childbed. A native chief, aged about fifty, told me that out of a tribe numbering 4000 souls, he could only recollect ten instances of women dying in childbed. This is about one death in three years out of 2000 women. The circumstances which caused death, the chief said, were hæmorrhage and cross-births; one medical man was called to a woman with an arm presentation, and another to a protracted labour, the result of a deformed pelvis. Child-bearing extends from fifteen to thirty-five years of age; but I have heard of a woman whose age, from certain known circumstances, must have been forty-seven when she gave birth to a child.

New Zealand women often give birth to large families. Twins and triplets occur; but three children born at one time have never been reared. I knew a woman who had fifteen children, all dead. When a woman has a protracted labour, it is assisted by violent pressure on the

abdomen. I saw a young female who was suffering from extensive ulceration of the muscles of the abdomen, which had come on after a protracted labour. It might have been produced by too violent pressure. Abortions occur, and are often caused and produced by pressure on the abdomen, or sitting over a native oven, such as will be afterwards described. I never heard of a woman with puerperal convulsions. Infanticide is generally perpetrated, the moment the child is born, by pressing the head between the thighs.

Child-bearing is usually easy with the New Zealanders. A New Zealand woman, the bearer of a burden, with a party of travellers, was confined on the road; after the birth of the child she walked four miles, and next day, fifteen. They rise almost immediately after the expulsion of the after-birth. As soon as a New Zealand woman finds her labour about to commence, she takes a blanket, and goes out into the open air, into a quiet retired place. If it is her first child, a woman attends her: after the first child they go out alone. After delivery, the woman proceeds to a stream and washes herself and her infant, and then returns home. During labour, the women kneel down, with their thighs apart, and having their hands resting on a tree or a stick. They hold their breath. Labour seldom exceeds two hours; generally, it is much shorter. Sickness after parturition is rare. The great ease of childbirth may be partly due to the pyramidal shape of the skull. Prolapsus uteri after childbirth is rare. In a large village, containing 400 women, there was only one woman who had this disease.

Epidemic Diseases.—According to all accounts, there have prevailed, during the last sixty or seventy years, three fatal epidemics; one called maripa, or makoko, broke out after the arrival of an European vessel in Mercury Bay, sixty years ago, and proved fatal to a great many persons. It had a dysenteric character, and caused death after a few days' illness. About five years after this, another pestilence broke out, called rewarewa, and so many died, that the living could not bury the dead. It commenced among the natives in the north part of the island, and had a dysenteric character. Tangara is the name of a pestilence which visited the island, and carried off great numbers, the nature of which I cannot ascertain. Since the settlement of the British Government in New Zealand, in 1840, there have been three epidemics—influenza, hooping-cough, and mumps. The first has been prevalent twice, and whole villages were prostrated by it. Hooping-cough and mumps are said to be new diseases; the former prevailed as an epidemic in 1847, the latter in 1851.

On the Method in which Diseases were treated by the New Zealanders.—The instinct to live, which is found so strong in the human breast, has made men, in all ages, endeavour to procure means to ward off death. Among the New Zealanders, the desire to live was as strong as among other races of men; yet the idea which existed in their minds, that all diseases are inflicted, directly or indirectly, by the gods for their sins, or by witchcraft, made them resort to prayers in place of physic for a restoration to health. I insert four out of the many prayers which were used for the treatment of disease, so as to convey some idea of their nature and style:*

* I am indebted for these prayers, and for their translation, to Mr. C. O. Davis, of Auckland, one of the few persons in New Zealand intimately acquainted with the language and modes

He Kopito.—Kopu nui, kopu roa, kopu takitaki, kopu whakaahu tena te ara te hamama na kawea kowhitia, pararitia, pupa, nau mai ki waho.

Translation of the above Prayer for Swollen Stomach.—Big belly, long belly, stretched belly, bursting belly: there is the passage open, take it hence, pluck it out.

He Korere.—Titi puru e, titi puru e, titi kōhea, titi maiami, e tena te titi ka titi, tena te puru ka puru, ko te puru ra tena, i purua ai te tupuna a Houtaiki.

Translation of the Prayer for Looseness of the Bowels.—Stop up the looseness, stop up the looseness, the purging will subside, the purging be stayed: there is the purging and there is the stopping up, for this is the remedy that stayed the malady of thy ancestor Houtaiki.

He Manawa.—Kei te manawa, kei taku, kei taku manawa kei te manawa whena, he manawa kaukau. Tina ki roto whena ki roto whaka-taka atu ki roto.

Translation of the above Prayer for Disease of the Heart, or Shortness of Breath.—It is in my heart, or it is my breath, and in thy breath it is in thy heart, and my heart; in the heart that is strenuous. Let it be over-powered inside, let it be strenuous inside,* let it be thrust back inside.

He Hono.—Tutakina i ou iwi, tutakina i ou toto, tutakina i ou mon-gameya tena te rangi, ka tutaki, tena te papa ka whena.

Translation of the above Prayer for a Sprained Back.—Close up your bones, close up your blood, close up your marrow, and be united as the heavens, and let the bones be strong as the earth.

Confidence was not, however, placed in such stupid compositions as the above, the exhibition of substances to cure diseases were occasionally resorted to; but the exact value of prayers and medicine will be at once appreciated when I state, that medicine, without the assistance of prayers to the gods, was totally inefficacious. New Zealanders have no idea of the circulation of the blood, nor of any of the proper functions of the different organs of the body. The head, although extremely sacred, is not supposed to contain the organ which produces the intellect. The stomach and bowels are believed to be the seat of some of the faculties of the mind, such as joy, fear, and sorrow. Every external part of the body has a name, and a good many of the internal parts. Their cannibal feasts gave them an opportunity of acquiring anatomical knowledge.

The New Zealand mode of treating surgical cases was often successful and judicious. For a broken bone, splints were made, from the bark of trees, in the shape of the part, and bandages were made from the flax plant. Dislocations were reduced. Sprains were treated with rest and shamooing. They never performed amputation. Scraped roots and leaves were applied hot for boils and sores. Abscesses and boils were opened, often long before they were ripe, and severe pressure applied to squeeze out the matter. The surgical instruments employed for opening

of thinking of the New Zealanders. The translations are literal, but it is a difficult subject, and few of the present generation of Maoris are acquainted with these prayers, or even the exact meaning of the words, and the difficulty is increased by the circumstance that many words have five or six meanings.

* This most probably refers to the god who is supposed to be in the heart of the sufferer.

abscesses were the sharp edge of a shell, a splinter of obsidian, or a sharp-pointed stick, or, a thorn. If a person received a wound, it was first washed, and then a plaster of mud applied to exclude the air, and this was allowed to remain until the wound was well; sometimes the wound, if small, was bruised with a stone, to excite bleeding, and afterwards held over the smoke of a fire of certain plants. The bleeding from a wound is sometimes checked by holding it over the smoke of a fire. For cutaneous diseases and sores, bathing in the hot sulphur and siliceous springs at Roturua Taupo, and at several other places, are reckoned very beneficial. For rheumatism, scarifications were made in the skin, or friction and shampooing with the fat of pigeons and whale oil, were used. In lumbago, stones were heated and rolled over the part. Change from one part of the country to another was an esteemed remedy for certain diseases, not for the climate, but to avoid the evil spirits which produced the sickness. Blistering a part was known, and it was done by the application of the leaves of certain plants—e. g., *clematis*. Local bleeding by scarifying the skin was known, but not venesection. Hæmorrhage from a wound was checked by bandages, and stuffing the wound with mud. Diseases which were particularly supposed to be caused from a devil, or an *atua*, living in the body, were occasionally starved out, *alias* the patient killed; at other times they were pressed out. Mr. Nicholas, a settler in the Thames, told me that, in 1848, he saw a person killed by an attempt to drive out the *atua*. A young, healthy New Zealander had a pain in his stomach: he was laid out naked on the ground, heavy baskets of stones were placed on his chest and stomach, on the top of which several persons sat for about a quarter of an hour; when they were removed the unfortunate man was dead—from suffocation, I presume, in consequence of the movements of the lungs being impeded. The evil spirit, in this case, was effectually expelled, but with it went the vital spark.

Vapour and hot-water baths were often used in the treatment of diseases. The vapour bath is made by digging a hole in the ground about two feet deep, and as large as is required; into this a number of very hot stones, about the size of a large orange, are put, a quantity of water is sprinkled over the stones, and over them a thick bed of the leaves of the plants which are supposed to be efficacious in the cure of the disease are spread. The patient either lies down on them, or sits on them. In the former case, the body is covered with a mat and a layer of earth, in the latter with a mat or blanket. The steam generated by the water on the hot stones rises through the leaves, envelopes the body, and produces a copious perspiration. This bath is often given; sometimes the patients are kept in it from sunrise to sunset, but generally for two or three hours. Vapour baths are constructed at Roturua over some of the hot rocks found in this volcanic district.

Mode of making the Hot-Water Bath.—This was not so often used. Captain Cook was wrong when he said the New Zealanders were ignorant of any way of boiling water; they have a highly ingenious way, which they used for giving hot baths. It is of heating stones very hot, and throwing them into cold water contained in a large wooden bowl. The water soon boils, and the heat is kept up by throwing in fresh hot stones. During the heating of the water, certain trees and barks are put

into it, which they impregnate with their properties. It is a medicated hot-water bath, and is chiefly used for cutaneous diseases.

Mode of recovering Persons who have been long under Water.—The body, immediately after being taken out of the water, is suspended, head downwards, over the smoke of a fire; if there is any life in the person, the smoke produces irritation in the nostrils, and sneezing. After this effect is produced, the body is put before a fire, and hot water poured down the throat.

Nominal List of Plants used as Medicine by the New Zealanders, and the Diseases for which they are supposed to be beneficial.—The expressed juice of the *Mesembryanthemum* (pig's ear), Nat. Ord. Ficoideæ, is used for boils. The leaf and root of the native flax, *Phormium tenax*, Ord. Asphodelæ, is used for ringworm, bandages, and various internal complaints. The bruised leaves steeped in water of a pepper-shrub, called *Horopito*, *Drimys auxillares*, Ord. Magnoliaceæ, is used as a remedy for cutaneous disease. The leaf of the *Kahikatoa* tree, *Dacrydium excelsum*, Ord. Coniferæ, is used in the form of decoction for urinary and other internal complaints, and in the steam bath. A decoction of the *Kareao*, Ord. Lorantheæ, is used for secondary symptoms, and to produce abortion; the young shoot is eaten to cure the itch. The leaf and bark of the *Kawakawa*, *Piper excelsus*, Ord. Piperaceæ, is used for cuts, wounds, cutaneous disorders, gonorrhœa, and in making vapour baths. A weak infusion of the *Kohekohe*, *Hartighsia spectabiles*, Ord. Meleaceæ, is used to stop the secretion of milk. Mosses found on trees, when dried and reduced to powder, are rubbed into the skin for cutaneous diseases. The *Kokowai* (red ochre) is used for sore heads and wounds. The leaves of the *Kopakopa*, something like the plantain, are used to bandage over ulcers, one side of the leaf draws, and the other heals. The tender leaves of the *Koromiko*, *Veronica segustrifolia*, Ord. Scrophularineæ, are bruised and applied as a poultice for ulcers, and especially venereal ulcers. The inner bark of the *Kowai* tree, *Edwardsia microphylla*, Ord. Leguminosæ, is used for itch. The bruised pith of the *Mamakū*, *Cyathea medullara*, Ord. Filices, is used as a poultice for swollen feet and sore eyes. The infusion of the bark of the *Miro*, *Podocarpus ferruginea*, Ord. Coniferæ, is drunk for pain in the stomach. An infusion of the root *Mouku*, a fern, is used as a wash for cutaneous complaints. The bark of the *Ngaio* tree, *Myoporum laetum*, Ord. Myoporineæ, is applied to ulcers and cutaneous eruptions. Charcoal from any wood was used in a state of powder for cutaneous eruptions. The pith of the *Ponga*, *Cyathea medullara*, Ord. Filices, was used as a poultice for cutaneous eruptions. The inner bark of the *Poroporo*, *Solanum lacineatum*, Ord. Solanææ, is used for the itch. The bark of the *Pukatea*, *Laurelia Novæ Zelandiæ*, Ord. Atherospermeæ, and the bark of the *Rata*, *Metrosideros robusta*, Ord. Myrtaceæ, are used for cutaneous diseases. The infused bark of the *Rimu*, *Dacrydium Cupressinum*, Ord. Coniferæ, is employed to dry up ulcers, and for wounds of all sorts. The water in which potatoes are boiled is used as a lotion for cutaneous diseases. The boiled blood from the ear of a dog is used as a remedy for spear-wounds. A decoction of the *Ti*, *Cordyline Australis*, Ord. Asphodelæ, is used for dysentery. The leaf, tender shoot, or pith of the *Tutu*, *Coriaria Sarmientosa*, Ord. Coriariæ, is used for dysentery; it

is very poisonous to animals and men; when a man has eaten the seeds and stalks of the tutu, and shows signs of poison, he is put under water for nearly half a minute, his stomach is by this means filled with water, he is then taken out and rolled on the ground, until he vomits. The expressed juice from the seeds of the tutu is a favourite drink of the New Zealanders, it is not poisonous, and is very pleasant. The juice of the leaf of the Puatanatana is used as a blister.

There are several other plants used by the natives to cure diseases. Different parts of the country use different substances. None of them are very active. Neither purgatives nor emetics are much used. A large proportion of the remedies are to cure cutaneous diseases, a very significant proof of their great frequency. There are few remedies for pectoral diseases, or indeed any internal or obscure malady. Much of the above information relative to the plants used in medicine is procured from the Rev. Mr. Taylor's 'Leaf from the Natural History of New Zealand.' The natural orders given, I have arranged from a French work, entitled 'Choix de Plantes de la Nouvelle Zélande Recueillies et Descrites, par M. E. Raoul, Chirurgien de première classe de la Marine Royal, 1846.'

Their Physicians.—From the New Zealanders' ideas about the pathology of diseases, the priests were their only useful medical men. When called to a sick person, the first thing done was to consult the gods as to the termination of the malady; there were several ways of doing this, but the most common was to pluck up a piece of fern, if the root came up clean and free from earth, a favourable prognosis was given; if, on the other hand, much earth adhered to the root, an opposite opinion was given. The sick were conveyed into the bush away from the village, and a small place built for them, and the place was tapued—when a sick person recovered, fern root or some other food was cooked, and a portion was set apart for the gods—the tapu was then removed. When a person was bewitched, the priests were summoned to drive the witch away; the incantations of the priests in such cases were generally performed near water, where they professed to see the spirit depart. In some cases the witch was transferred from one person to another. I have seen a woman have nothing the matter with her, almost die from a belief in witchcraft; she rapidly recovered on a priest visiting her, and repeating a short prayer. The tapuing of the sick, and their removal from among the healthy, is merely an extension of our laws for the prevention of contagious disease.

ART. IV.

Note on the Induction of Sleep and Anæsthesia by Compression of the Carotids. By ALEXANDER FLEMING, M.D., Professor of Materia Medica, Queen's College, Cork.

WHILE preparing a lecture on the mode of operation of narcotic medicines, I thought of trying the effect of compressing the carotid arteries on the functions of the brain. I requested a friend to make the first experiment on my own person. He compressed the vessels at the upper part of the neck, with the effect of causing immediately deep sleep. This experiment has been frequently repeated on myself with success, and I have made several cautious but successful trials on others. It is sometimes difficult

to catch the vessels accurately, but once fairly under the finger, the effect is immediate and decided.

There is felt a soft humming in the ears, a sense of tingling steals over the body, and, in a few seconds, complete unconsciousness and insensibility supervene, and continue so long as the pressure is maintained. On its removal, there is confusion of thought, with return of the tingling sensation, and in a few seconds consciousness is restored. The operation pales the face slightly, but the pulse is little, if at all, affected. In profound sleep, the breathing is stertorous, but otherwise free. The inspirations are deeper. The mind dreams with much activity, and a few seconds appear as hours, from the number and rapid succession of thoughts passing through the brain. The experiments have never caused nausea, sickness, or other unpleasant symptom, except, in two or three instances, languor. The period of profound sleep, in my experiments, has seldom exceeded fifteen seconds, and never half a minute.

The best mode of operating is to place the thumb of each hand under the angle of the lower jaw, and, feeling the artery, to press backwards, and obstruct the circulation through it. The recumbent position is best, and the head of the patient should lie a little forwards, to relax the skin. There should be no pressure on the windpipe.

The internal jugular vein must be more or less compressed at the same time with the carotid artery; and it may be thought that the phenomenon is due, wholly or in part, to the obstructed return of blood from the head. I am satisfied that the compression of the artery, and, not of the vein, is the cause. The effect is most decided and rapid when the arterial pulsation is distinctly controlled by the finger, and the face loses somewhat of its colour; and, on the other hand, is manifestly postponed and rendered imperfect when the compression causes congestion of the countenance.

This mode of inducing *anæsthesia* is quick and certain. The effects diminish immediately when the arteries are relieved from pressure, and are not liable to increase, as happens sometimes with chloroform and ether, after the patient has ceased to respire their vapours. So far as my experience goes, it has shown no tendency to cause faintness; and usually, after its employment, no unpleasant feeling whatever remains.

I think it may be found useful as a remedial agent in certain headaches, tetanus, asthma, and other spasmodic diseases, and to prevent pain in such small operations as the extraction of a tooth or the opening of an abscess. Whether the compression can be continued *with safety* sufficiently long to make it available in larger operations, has to be ascertained. But, whatever be the practical value of this observation, it is at least interesting as a physiological fact, and may be the means of throwing light on the causes of ordinary, medicinal, and hypnotic sleep, and of coma. Some facts encourage the supposition that the circulation of the brain is languid in ordinary slumber, and the etymology of the word carotid shows the ancient belief in the dependence of deep sleep on some interference with the passage of the blood through these vessels; and it is not an unreasonable conjecture, that hypnotic sleep may be sometimes caused or promoted by the contracted muscles and constrained position of the neck compressing the carotid arteries, and diminishing the supply of blood to, and pressure on, the brain.

PART FOURTH.

Chronicle of Medical Science.

REPORT ON THE PROGRESS OF ANIMAL CHEMISTRY DURING THE YEARS 1852-3-4.

By GEORGE EDWARD DAY, M.D., F.R.S.,
PROFESSOR OF MEDICINE IN THE UNIVERSITY OF ST. ANDREWS.

THE following are the most important works on physiological and pathological chemistry which have appeared during the last three years.

1. MOLESCHOTT: *Der Kreislauf des Lebens. Physiologische Antworten auf Liebig's Chemische Briefe.*—Mainz, 1852.
2. ROBIN ET VERDEIL: *Traité de Chimie Anatomique et Physiologique, ou des Principes immédiats Normaux et Morbides qui constituent le Corps de l'Homme et des Mammifères.* 3 vols. 8vo. Accompagnés d'un atlas de 45 planches gravées, en partie coloriées.—Paris, 1853.
3. HEINTZ: *Lehrbuch der Zoochemie.*—Berlin, 1853.
4. SCHLOSSBERGER: *Lehrbuch der Organischen Chemie mit besonderer Rücksicht auf Physiologie und Pathologie, n. s. w. Dritte durchaus umgearbeitete und vermehrte Auflage.*—Stuttgart, 1854.
5. SCHLOSSBERGER: *Erster Versuch einer Allgemeinen und Vergleichenden Thier-Chemie. Erste Lieferung. Beginn der Vergleichenden Gewebschemie.*—Stuttgart, 1854.
6. GORUP-BESANEZ: *Anleitung zur Qualitativen und Quantitativen Zoochemischen Analyse. Zweite umgearbeitete und vermehrte Auflage.*—Nürnberg, 1854.
7. BECQUEREL ET ROUVER: *Traité de Chimie Pathologique appliquée à la Médecine Pratique.*—Paris, 1854.
8. LEHMANN: *Lehrbuch der Physiologischen Chemie. Zweite Auflage (Zweite Umarbeitung).* 3 Bde.—Leipzig, 1853.
9. DAY: *A Translation of LEHMANN'S Physiological Chemistry, in 3 vols.*—London. (Printed for the Cavendish Society.) 1851—1854.
10. FUNKE: *Atlas der Physiologischen Chemie, zugleich als Supplement zu C. G. LEHMANN'S Lehrbuch der Physiologischen Chemie.*—Leipzig, 1853.
11. FUNKE: *Atlas of Physiological Chemistry. Being a Supplement to LEHMANN'S Physiological Chemistry.*—London. (Printed for the Cavendish Society.) 1853.
12. LEHMANN: *Handbuch der Physiologischen Chemie.*—Leipzig, 1854.

MOLESCHOTT'S volume is evidently intended as an attack by the Dutch chemists upon the special doctrines of Liebig and the Giessen school. We need do little more than mention the subjects of his different letters. 1. Revelation and the laws of nature. 2. The sources of human knowledge. 3. The indestructibility of matter. 4. The growth of plants and animals. 5. The earth in its connexion with the production of plants and animals. 6. The circulation of matter. (The author in this letter argues strongly in favour of Mulder's views regarding the humus theory. Most of our readers are probably aware that Mulder believes that the hemic-acid group, in combination with ammonia, exerts a considerable influence on the nutrition of plants; while Liebig holds that the main use of manure is

referable to the inorganic substances which it contains.) 7. Plants and the soil. 8. Plants and animals. 9. Nutrition and respiration. (In this letter, Liebig's well-known division of nutriment into plastic, and respiratory food is very severely criticised.) 10. The development of the food into blood, &c. (Moleschott here gives us his own division of food into albuminous bodies, fat-formers, fats, and salts). 11. The ashes (or mineral constituents) of man and animals. 12. The disintegration of the animal tissues. (This letter contains an excellent popular account of the subject of which it treats.) 13. The disintegration of the vegetable tissues. 14. The development of heat in plants and animals. (Liebig's view, that the development of animal heat is solely due to a process of oxidation, is here opposed with much ingenuity.) 15. The gradual development of matter. (The formation of organic matter in plants, from inorganic constituents, and the various forms of decomposition of organized structures after death, are here considered.) 16. Matter rules man. (In the letter with this strange heading, the author attempts to demonstrate the influence of food—or, in other words, matter—on the activity and power of man; he shows, by numerous examples, the injurious effects of a solely animal or solely vegetable diet, and demonstrates the necessity of a mixed diet in the case of man. 17. Force and matter. 18. Thought. 19. The will. 20. Vital economics.

This little work is well deserving of a careful perusal; and we should be glad to see it translated, as a companion volume to Liebig's 'Chemical Letters.'

Robin and Verdeil's treatise is of a most elaborate description. We have already reviewed it at such length, that it is unnecessary again to enumerate its contents.

Taking the work as a whole, we may state as our opinion, that it will, for many years to come, prove of invaluable aid to the lecturer or writer on animal chemistry, but that it is too cumbrous and elaborate in its structure for the student or general medical reader. It is honourably distinguished from all other French chemical works with which we are acquainted, by the familiarity with English and German literature which its authors exhibit; and it is accompanied by an admirable Atlas.

Heintz's '*Lehrbuch der Zochemie*,' a thick volume of upwards of eleven hundred pages, next claims a brief notice. It is divided into two parts, of which the first occupies 833 pages, and is devoted to the consideration of the constituents of the animal body. This part is arranged as follows:

- I. *Inorganic substances occurring in the animal organism.*
- II. *Organic constituents of the animal body.*

These are divided as follows:

- I. Basic compounds.
- II. Acid compounds.
- III. Indifferent substances.
- IV. Substances whose nature and properties are not as yet accurately known, as, for instance, the extractive matters.

The second part is devoted to "the methods of analysing animal substances." The Introduction, which includes two sections on the recognition of seminal and blood spots, is followed by general remarks on the qualitative and quantitative analysis of animal matters; and these are succeeded by descriptions of all the best methods of analysing individual substances—namely, the bones, urine and urinary sediments, blood, milk, bile, and animal concretions. This part concludes with sections upon the analytical separation of the gases occurring in the animal body; upon the qualitative and quantitative determination of the carbon, hydrogen, nitrogen, sulphur, and phosphorus in organic substances; with tables for the calculation of analytical results.

In the first part, the different substances entering into the composition of the body are very fully and accurately considered in reference to their history and occurrence, their most essential chemical properties and compounds, their products of decomposition, and their qualitative and quantitative determination. It is to be regretted that Heintz has not imitated Lehmann's admirable example of entering into the consideration of the importance of the various substances in the animal economy, and their metamorphoses within the organism. The author's high reputation as an analytical chemist renders it unnecessary for us to allude to the excellence of the second part. Like the work of Robin and Verdeil, it contains much that might have been omitted without detriment, and it omits many subjects—as, for instance, respiration, digestion, nutrition, &c.—which we should have been glad to have found in it.

Schlossberger's '*Lehrbuch der Organischen Chemie*' has passed through three editions in the course of four years. It is a work of great value, but, as its title implies, it is not so exclusively devoted to the subject of this Report as the preceding volumes.

The same author's '*Erster Versuch einer Allgemeinen und Vergleichenden Thier-Chemie*' is not yet sufficiently advanced to enable us to express an opinion on its general merits. We shall, however, have occasion to notice it in a future page in connexion with the chemistry of "cartilage and bone," and of "the nervous system."

Gorup-Besanez published the first edition of his '*Anleitung zur Zoochemischen Analyse*' in 1850; the second edition, now before us, has been carefully revised, and received numerous additions. It is divided into a *general* and a *special* part.

The *first* or *general* part treats of the operations employed in chemical investigations, of the reagents made use of, of the necessary apparatus, of the composition, properties, and recognition of the compounds occurring in zoo-chemical investigations.

The *second* or *special* part enters fully into the analysis of the blood (the various methods of Scherer, Becquerel and Rodier, Figuier and Dumas, and Schmidt, being given), the urine and urinary sediments, the milk, the bile, serous or albuminous fluids (chyle, lymph, pus, liquor amnii, transudations), the digestive fluids, expectorated and vomited matters and the excrements, the bones, concretions, the semi-solid tissues, the expired air, and the ash of animal substances.

The mode of arrangement and the execution of this volume are equally admirable, and we are glad to be enabled to state that an English translation of it is in preparation.

Becquerel and Rodier's '*Traité de Chimie Pathologique*' presents no claim whatever to our favourable consideration; the authors seem to be profoundly ignorant of all chemical investigations that have been carried on out of France, and their book does not in any respect represent the present state of animal chemistry.

Lehmann's '*Lehrbuch der Physiologischen Chemie*,' and Funke's '*Atlas*' to it, are too well known to the English reader, through the translations issued by the Cavendish Society, to require any comment. The readers of this journal are well aware of the very high estimate in which the '*Lehrbuch*' is held by the reviewers to whom this department of medical science is entrusted.

Lehmann's '*Handbuch der Physiologischen Chemie*' is a condensation of his '*Lehrbuch*.' It includes, in a moderate octavo volume, the positive facts of physiological chemistry, in so far as they are yet established, given in the most compressed form, and excludes all discussions on disputed points, and all names and references. Our only objection to it is its brevity.

Lehmann in both his works divides physiological chemistry into three parts:

1. The theory of the chemical substrata of the animal body; or, zoo-chemistry in the strict sense of the word.

II. The theory of the animal juices and tissues (phlegmato-chemistry and histo-chemistry).

III. The theory of zoo-chemical processes—that is to say, the chemistry of the metamorphosis of matter, of nutrition, and of secretion.

We shall adopt Lehmann's arrangement in the following pages; and in considering the additions that have been recently made to our knowledge of the "organic substrata of the animal organism," shall commence with the

ORGANIC NON-NITROGENOUS ACIDS.

Oxalic Acid.—Lehmann,² in the new edition of his "Lehrbuch," points out that oxalate of lime may possibly be formed in the urine after its emission, during the acid fermentation which has been so well described by Scherer. We know that there is a close connexion between the excretion of uric acid and the formation of this salt, from the circumstance that, in most specimens of urine, both sedimentary and non-sedimentary, oxalate of lime cannot be recognised by the microscope so long as the fluid is fresh, but as soon as crystals of uric acid present themselves, crystals of oxalate of lime, in varying numbers, may also be detected; indeed, we generally find that, in morbid urine, the abundance of these crystals is proportional to the rapidity with which the free uric acid separates. Since uric acid, when acted upon by certain oxidizing agents, may be decomposed by the chemist into urea, allantoine, and oxalic acid, there is no difficulty in the supposition that a part of the uric acid is decomposed during this acid urinary fermentation, and that oxalic acid is thus produced; and this view is much strengthened by the recent observation of Ranke,³ that urate of soda, when exposed with yeast to a temperature of about 90° Fahr., became converted in a few days into oxalic acid and urea.

In connexion with this subject, we must notice an essay 'On the History of the Development of Oxaluria,' by Dr. Beneke,⁴ as well worthy of an attentive perusal.

Formic and acetic acids have been detected by Scherer in the acid fluid expressed from muscular flesh,⁵ and from the spleen,⁶ and likewise in the blood in leucæmia.⁷ Schottin⁸ has found formic acid and (in a lesser quantity) acetic acid in the normal sweat; and Campbell⁹ has found formic acid in vomited matters, in the blood, and in the urine.

Butyric acid has been detected (and not as a mere product of decomposition) in the sweat, by Schottin,¹⁰ who believes that he has also found *metacetic acid* in the same fluid; it has likewise been found in the gastric juice by Gruenewaldt.¹¹

Valerianic acid in association with leucine has been found in the urine in typhus fever, by Frerichs;¹² the leucine had probably undergone a partial decomposition into valerianate of ammonia.

The *non-volatile fatty acids* will be noticed when we consider the *fats*.

Lactic acid has been detected by Lehmann¹³ in the juice of the smooth muscles; and by Scherer¹⁴ in the juice of the spleen, and in leucæmic blood. Neither

² Lehrbuch der Phys. Chem., vol. i. p. 43.

³ Journ. f. pr. Chem., vol. lvi. p. 16.

⁴ Zur Entwicklungsgeschichte der Oxalurie. Ein dritter Beitrag zur physiologischen Heilkunde. Von Dr. F. W. Beneke. Göttingen, 1852.

⁵ Ann. der Chem. und Pharm., vol. lxx. pp. 196—201.

⁶ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 298.

⁷ Ibid., p. 321.

⁸ Arch. für Phys. Heilk., vol. xi. pp. 73—104.

⁹ Chem. Gaz., vol. xi. p. 310; and vol. xii. p. 52.

¹⁰ Op. cit.

¹¹ Succu Gastrici Humani Indoles Physica et Chemica. Diss. Inaug. Dorp. Liv. 1:53.

¹² Schmidt's Jahrbücher, vol. lxxiv. p. 146.

¹³ Lehrbuch der Phys. Chem., vol. i. p. 107.

¹⁴ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 299.

Schottin nor Lehmann could find it in the sweat, although Favre¹⁵ not only maintains that it is present, but professes to have determined its quantity. Robin and Verdeil¹⁶ have recently found lactate of lime in large quantity in the urine of the horse. The discussion between Lehmann and C. Schmidt regarding the presence of this acid in the gastric juice, can hardly be said to be satisfactorily settled. "There can be no doubt (says Lehmann), when we consider Schmidt's well-known accuracy as a chemist, that, in the cases which he analysed, the gastric juice contained no lactic acid, and that it was replaced by free hydrochloric acid. . . . I have as yet been unable to determine the conditions under which it occurs in the gastric juice, and those under which it is absent."

Schorer¹⁷ has recently published an excellent memoir, 'On the Recognition of small quantities of Lactic Acid in Animal Matters.'

Hippuric acid (or, rather, hippurate of soda) has been found by Verdeil and Dollfus¹⁸ in the blood of the ox; and Hervier¹⁹ has also found it in morbid human blood. The hippurates of soda and potash have been found by Robin and Verdeil in the urine of the pig, but they could not detect the presence of these salts in the urine of the dog. Duchek²⁰ found hippuric acid in his own urine whenever he had taken green plums (the ripe fruit of *Prunus domestica*). Their analysis, however, revealed the presence of a considerable quantity of benzoic acid. The hippuric acid began to appear about seven or eight hours after he had eaten the fruit, and ceased to appear from three to five hours later.

Schlossberger²¹ has detected hippuric acid in the cutaneous scales in a well-marked case of ichthyosis. He digested a quarter of a pound of the scaly matter for fourteen days in alcohol of 80°, and obtained a deep yellow tincture, which, when carefully evaporated, yielded a thick syrup. A microscopic examination showed the presence of numerous fat-globules, and of beautiful tablets of cholesteroline; and on treating this syrup with a comparatively small quantity of spirit (so as not to take up much fat), the filtered yellow solution yielded on spontaneous evaporation, club-like crystals, mostly arranged in twos in oblique crosses; these crystals resolved themselves under the microscope into conical tufts of four sided prisms, presenting the greatest similarity with the groups of hippurate of lime depicted by Robin and Verdeil in their 'Atlas.' No trace of lime could, however, be detected in these crystals, which were acid, difficult of solution in cold water and in ether, less so in alcohol, and readily soluble in boiling water. They remained unchanged in mineral acids, but dissolved in a solution of potash; when heated in a glass tube, they readily fused into a yellow oil, and then developed white vapours, which condensed in a solid form on the walls of the tube. On the application of a stronger heat an unmistakable odour of prussic acid was recognised, and a blistered carbonaceous residue was left, which burned away on a platinum spatula without leaving any residue. The aqueous solution of the crystals yielded, with nitrate of silver and a drop of ammonia, a white curdy precipitate, like chloride of silver, which on boiling dissolved in water.

From these reactions, Schlossberger felt assured that the crystals were actually composed of hippuric acid, which has hitherto been found only in the urine and the blood; and this result is the more singular, since Schottin, in the memoir 'On the Sweat,' to which we have already referred, found that benzoic acid, when administered to a healthy man, appeared unchanged in the sweat, and not in the form of hippuric acid, as in the urine.

It has been long known that benzoic acid becomes converted in the human body into hippuric acid, in which form it is excreted by the kidneys. It has been shown

¹⁵ Arch. Gen. de Méd., 1853, cinquième série, vol. ii. pp. 1—21.

¹⁶ Traité de Chimie Anat., vol. ii. p. 377.

¹⁷ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. iv. p. 285.

¹⁸ Traité de Chimie Anat., vol. ii. p. 446.

¹⁹ Gaz. Méd. de Paris, 1853, No. 5.

²⁰ Prag. Viertelj., 1854: quoted in p. 270 of this volume.

²¹ Ann. der Chem. und Pharm., vol. xc. p. 378.

by the later experiments of Erdmann and Marchand,²² that cinnamic acid undergoes a similar change; and this has led to the inquiry, whether other acids of the benzoic acid group, as, for instance, toluyllic and cunic (or cuminic) acids, underwent the same change. The recent observations of Hofmann²³ and Ranke²⁴ show that this is not the case.

Uric acid has been found by Scherer²⁵ in considerable quantity, as a normal constituent, in the juice of the spleen. Mr. Henry Gray,²⁶ being anxious to confirm Scherer's observation, worked in one experiment on the spleens of twenty-five oxen, but wholly failed in detecting this substance. (Mr. Gray's chemical experiments were, we believe, superintended by Dr. Noad.)

Cloëtta²⁷ (in vainly searching for pulmonic acid) found uric acid in the pulmonary tissue. He examined the lungs of six oxen, and always obtained this substance from the water-extract. The following was his method of procedure: the chopped lung, after remaining in distilled water for twenty-four hours, was exposed to very strong pressure. The albumen and pigment were removed by boiling, and the filtered fluid was then treated with baryta water till no additional precipitate was formed. The filtrate was now slowly evaporated on the water-bath, the baryta compounds that formed on the surface being removed as they appeared. When the volume of the fluid did not amount to much more than 50 c. c.,²⁸ a little acetic acid was added; and after some time a precipitate was formed, which exhibited the chemical reactions and the well-known crystalline forms of uric acid.

• He obtained 60 milligrammes of uric acid from the water-extract of one ox-lung.

Reference has been made, in a previous page, to Dr. Garrod's observations on uric acid in the blood and blister fluid, and its absence in the sweat in one case.

Arppe²⁹ has published an improved method of obtaining uric acid from pigeons' dung.

Pneumic (or *pulmonic*) acid probably belongs to the conjugated nitrogenous acids. This acid, of which as yet we know very little, was discovered by Verdeil³⁰ in the tissue of the lungs. The minced pulmonary tissue is stirred with water, and exposed to strong pressure; the decanted acid fluid is heated in order to coagulate the albumen, and is then filtered, neutralized with baryta water, and evaporated to three-fourths of its volume. After the removal of albuminous and some other matters by sulphide of barium, we evaporate the fluid till crystals of sulphate of soda are formed; we then add a little sulphuric acid, and boil with alcohol. The acid gradually separates from the alcoholic solution on cooling; it crystallizes in oblique rhombic prisms, is extremely glistening, and refracts light strongly, loses no water of crystallization at 212° Fahr., but at a higher temperature decomposes. It dissolves readily in water; is insoluble in cold, but dissolves in boiling alcohol; is insoluble in ether, forms crystallizable salts with bases, and contains not only carbon, hydrogen, and oxygen, but also nitrogen and sulphur.

It appears to be a constituent of the lungs of all mammals. Verdeil obtained about five centigrammes from the lungs of a perfectly healthy woman who was guillotined. Morbid conditions appear to occasion an augmentation rather than a diminution of this substance: thus, a single lung from a man with general pneumonia in its second stage, yielded rather more than the two lungs of the guillotined woman. It appears to be formed in the substance of the lung itself, and probably bears much the same relation to the pulmonary tissue that creatine does to muscle. Verdeil believes, that by decomposing the carbonates of the

²² Journ. f. Pr. Chem., vol. xxxv. p. 307.

²³ Ann. der Chem. und Pharm., vol. lxxiv. p. 342.

²⁴ Journ. f. Pr. Chem., vol. lvi. p. 3—6.

²⁵ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 299.

²⁶ Gray on the Structure and Use of the Spleen, p. 209. London, 1854.

²⁷ Arch. für Path. Anat., vol. vii. p. 168.

²⁸ c. c. indicate cubic centimetres.

²⁹ Ann. der Chem. und Pharm., vol. lxxxvii. p. 237.

³⁰ Traité de Chimie Anat., vol. ii. p. 460.

blood with which it comes in contact, it contributes very considerably to the evolution of carbonic acid, and is thus an important factor in the respiratory process. This, however, requires confirmation.

Two new acids have been obtained by Marcet from human urine. His own description of them may be found in vol. xi. p. 371, of this Review.

Cynuric acid is the term given by Liebig³¹ to an acid which he has discovered in the urine of dogs, and which seems to take the place of uric acid. He was only able to obtain it in a comparatively small number of cases, in which it was deposited, in the form of a very minute precipitate, after the urine had stood for some days (and, we presume, after a few drops of some acid had been added). It contains little or no nitrogen. For further chemical particulars regarding it we must refer to the original memoir.

BASIC BODIES.

Nitrogenous Basic Bodies next claim our attention. Those which do not contain oxygen, although of very high interest in a chemical point of view, scarcely come within the scope of this Report, except in so far as some of them have been recently found to occur, more frequently than was formerly supposed, as products of the decomposition of animal matter. Thus, for instance, trimethylamine ($3\text{C}_2\text{H}_5 + \text{N}$) has been found in herring-brine by Wertheim,³² who, however, mistook it for propylamine ($\text{C}_3\text{H}_7 + \text{N H}_2$) till Hofmann and Winkles³³ recognised its true nature; and, by Buckheim,³⁴ in the spirit in which anatomical preparations had been long kept. Winckler³⁵ obtained a fluid containing propylamine by distilling fresh urine with lime. Schlossberger³⁶ believes that the poison which is occasionally present in foreign sausages is due to the development of one of these alkaloids.

Of the nitrogenous basic bodies containing oxygen we must notice the following:—Leucine, tyrosine, hypoxanthine, xanthocystine, lienine, thymine, myeline (?), creatine, creatinine, urea, allantoin, and taurine.

Leucine is best prepared, according to Zölliker,³⁷ by boiling the ligamentum nuchæ of the ox (or any other elastic tissue) for forty-eight hours in sulphuric acid diluted with one and a half times its bulk of water, neutralizing with milk of lime, boiling the pulpy mass which is thus formed, and filtering. The salts of lime, which become deposited during evaporation on the sand-baths, must be removed as much as possible; on further evaporation on the water-bath, crystals of leucine are separated in abundance.

Leyer and Koller³⁸ have shown that albumen, fibrine, caseine, horn, feathers, hedgehog-spines, hairs, and the elytra of the cockchafer, yield, as products of their decomposition, leucine and another base, to which we shall presently refer, tyrosine, when treated with dilute sulphuric acid.

Reinhold Hofmann³⁹ has proposed a test (or rather a modification of an old test by Braconnet) for distinguishing whether leucine is free from tyrosine, a basic body which is often associated with it. Pure leucine is precipitated by nitrate of protoxide of mercury in white flakes, without any reddening of the supernatant fluid; any reddening that may occur being due to the presence of tyrosine.

³¹ Ann. der Chem. und Pharm., vol. lxxxvi. p. 125; or Chem. Gaz., vol. xi. p. 3 §. 1853.

³² Journ. für Pr. Chem., vol. lli. p. 435.

³³ Ann. der Chem. und Pharm., vol. lxxxiii. p. 116; Quart. Journ. Chem. Soc. vol. vii. p. 63. 1854.

³⁴ Lehmann's Lehrbuch, vol. i. p. 129.

³⁵ Buchner's Repert. für Pharm.: quoted in Chem. Gaz., vol. x. p. 320. 1852.

³⁶ Arch. für Physiol. Heilk., vol. xi. p. 742.

³⁷ Ann. der Chem. und Pharm., vol. lxxxii. p. 168.

³⁸ Journ. für Pr. Chem., vol. lviii. p. 273; Ann. der Chem. und Pharm., vol. lxxxiii. p. 362; and Chem. Gazette, vol. xi. p. 346. 1853.

³⁹ Ann. der Chem. und Pharm., vol. lxxxvii. p. 123.

Liebig⁴⁰ states that he has found leucine in the decoction of the liver of the calf; and according to Robin and Verdeil⁴¹ it exists in the pulmonary tissue and in the blood; they have not, however, attempted to determine it quantitatively. Frerichs and Städeler⁴² have found it in association with tyrosine in the human liver, in certain diseased states; these bodies have also been found in the blood of typhus and variolous patients; and leucine has been found in the urine in these cases.

Gössmann⁴³ has succeeded in artificially converting thialdine ($C_{12}H_{13}N S_4$) into leucine ($C_{12}H_{13}N O_4$).

Tyrosine ($C_{10}H_9NO_3$) is formed, along with leucine and other compounds not yet investigated, when albuminous or horny bodies are decomposed either by acids, alkalis, or putrefaction. Piria⁴⁴ has recently described a simple method of obtaining this substance from horse shavings.

We have already referred to the sources of tyrosine discovered by Leyer and Köller, who give in their memoir full particulars of the experiments which they performed.

Piria gives the following test for the detection of tyrosine, which is available even when the quantity does not exceed five or six milligrammes. It is based upon the fact, that when sulphuric acid acts on tyrosine, there is a formation of tyrosine-sulphuric acid, whose neutral salts yield a dark violet colour with perchloride of iron. Place a little tyrosine (a few milligrammes) on a watchglass, moisten it with one or two drops of sulphuric acid; after allowing the glass to stand covered for half an hour, dilute the mixture with water, saturate it, when heated, with carbonate of lime, and filter; if we now add to the filtrate a few drops of a solution of perchloride of iron in which there is no free acid, we at once obtain a very rich violet colour, which is very similar to that which this reagent induces with sulleyous acid.

Reinhold Hofmann⁴⁵ gives the following test for tyrosine. Its solution at a boiling heat is precipitated by nitrate of protoxide of mercury in red flakes, and the supernatant clear fluid assumes a dark rose-red colour, which, however, disappears after a time with a deposition of red flakes. The red colour is permanently destroyed on heating the mixture with a little nitric acid, the tint not being restored by subsequent neutralization. By means of this colour, one part of tyrosine may be detected in more than 1000 of fluid. If an excess of acid be present in the nitrate of mercury, there is neither colour nor precipitation.

Hypoxanthine ($C_{10}H_4N_4O_2$) has been found by Gerhard⁴⁶ (a pupil of Scherer's) in the blood of oxen, and by Scherer⁴⁷ himself, in larger quantity, in the blood of a patient with leucæmia.

Mr. Gray⁴⁸ failed in confirming Scherer's observations regarding the occurrence of this substance in the spleen.

Xanthocystine is the term applied by Chevallier and Lassaigne⁴⁹ to a substance which they extracted from the milary tubercles in a dead body that had been buried for two months. It was insoluble in water and alcohol, but dissolved in ammonia and in the mineral acids; the ammoniacal solution deposited minute white granules on evaporation; hexagonal tablets separated from the acid solutions on evaporation; the substance did not fuse on heating, but puffed up, became

⁴⁰ Letters on Chemistry, third edition, p. 354; and Gregory's Handbook of Organic Chemistry, third edition, p. 422. 1852.

⁴¹ Traité de Chimie Anat., vol. iii. p. 420.

⁴² Quoted in p. 273 of the present volume.

⁴³ Ann. der Chem. und Pharm., vol. xc. p. 184.

⁴⁴ Ibid., vol. lxxxii. p. 251.

⁴⁵ Ann. der Chem. und Pharm., vol. lxxxvii. p. 124.

⁴⁶ Verhandl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 299.

⁴⁷ Ibid., p. 324.

⁴⁸ Op. cit., p. 209.

⁴⁹ Journ. de Chim. Méd., troisième série, vol. vii. p. 208; and quoted in Lehmann's Lehrbuch, vol. i. p. 146.

yellow and black, developed an odour of burned horn, and gave off alkaline vapours. The investigation was not carried any further.

Lienine is a new nitrogenous substance, obtained by Scherer⁵⁰ from the fluid of the spleen. It contains no sulphur, and consists of C 53.71, H 8.95, N 4.82, and O 32.52.

Thymine is a new organic base, discovered by Gorup-Besanez⁵¹ in the tissue of the thymus gland. It was obtained by thoroughly extracting minced thymus glands (from the calf) with cold water, the fat having been previously removed. The fluid thus obtained was freed from albumen by boiling in the water-bath, was then treated with heavy water as long as any turbidity ensued, was filtered, and carefully evaporated on the water-bath to the consistence of a syrup. The residue, when extracted with spirit, and allowed to evaporate slowly, deposited impure thymine in wart-like granular masses, which was obtained in a state of purity by repeated solution in boiling alcohol, from which it was precipitated on cooling and evaporation. Only 200 milligrammes were yielded by twenty-one pounds of glandular structure. It was obtained pure, and in a crystalline form, by repeated solution in hot alcohol.

It has not yet been submitted to ultimate analysis, but the following properties of it have been ascertained:

Thymine forms slender snow-white crystals, grouped concentrically; it is perfectly inodorous and tasteless. It is readily soluble in water, and in boiling alcohol, difficult of solution in cold alcohol, and insoluble in ether. The concentrated aqueous solution exerts no action on test-paper. When quickly heated on platinum foil it burns with a bluish and not very luminous flame, and leaves no residue; when heated in a glass tube it yields a sublimate.

It dissolves in potash without any development of ammonia, and is likewise soluble in caustic ammonia. It forms crystalline compounds with acids, and a double salt with bichloride of platinum; the latter crystallizes in bright yellow octohedra, which are insoluble in alcohol, but dissolve tolerably easily in water.

The substances which it most closely resembles are sarcosine and alanine. It may be distinguished from sarcosine by its crystalline form, its tastelessness, and the form and character of its compounds with acids (hydrochloric and sulphuric), and with bichloride of platinum; while it differs from alanine in its not having a sweet taste, in the insolubility of its platinum compound in alcohol, and in the character of its salts.

Myeline is the name given by Virchow⁵² to a substance which he has discovered in diseased lungs, in an hepatic cyst, in healthy and diseased spleens, &c., &c. He has given it this name from its resemblance to the nerve-medulla. Its chemical nature seems extremely doubtful.

Creatine has been found by Lehmann⁵³ in the smooth muscles of the stomach of the pig, and by Siegmund⁵⁴ in the muscular substance of the uterus in the eighth month of pregnancy. Verdeil and Marcet⁵⁵ have found it in the blood.

A most elaborate account of the modes in which creatine crystallizes is to be found in Robin and Verdeil,⁵⁶ and ten figures of the crystals are given in their 'Atlas.' The crystalline characters are also given in Funke's 'Atlas.'

Dessaignes⁵⁷ has examined some of the products of the transformation of creatine, and has attempted to determine its rational formula.

⁵⁰ Verhändl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 299.

⁵¹ Ann. der Chem. und Pharm., vol. lxxxix. p. 115; and Chem. Gaz., vol. xii. p. 127, 1854; or Gorup-Besanez, Anleitung zur Zoochem. Analyse, p. 119, 1854.

⁵² Arch. für Path. Anat., vol. vi. p. 562.

⁵³ Lehrbuch der Phys. Chemie, vol. i. p. 150.

⁵⁴ Verhändl. der Phys.-Med. Ges. zu Würzburg, vol. ii. p. 50.

⁵⁵ Traité de Chimie Anat., vol. ii. p. 480.

⁵⁶ Op. cit., vol. ii. pp. 483—488.

⁵⁷ Comptes Rendus, May 8, 1854; and Chem. Gaz., vol. xii. p. 201, 1854.

Creatinine has been found by Verdeil and Marcet⁵⁸ in the blood; the same chemists have detected it in the urine of the horse, the pig, and the sheep; and Socoloff⁵⁹ has found it in the urine of the horse and the calf.

Much original matter regarding the forms in which it crystallizes will be found in Robin and Verdeil's work.

Urea.—Several important memoirs have been recently published regarding this substance; amongst which we must especially notice Liebig's⁶⁰ memoir "On certain Urea-compounds, and a New Method of Determining the Chloride of Sodium and the Urea in Urine;" and Bischoff's⁶¹ treatise 'On Urea as a Measure of the Metamorphosis of Tissue.'

We have not space to enter fully into the description of Liebig's⁶² volumetric method for the determination of the urea and the chloride of sodium in the urine. It rests essentially upon the three following facts:

1. Nitrate of protoxide of mercury at once induces a thick white precipitate in a solution of urea; this precipitate does not occur on the addition of a solution of chloride of mercury (corrosive sublimate). On mixing a solution of urea with chloride of sodium, and adding gradually, in small portions, a dilute solution of nitrate of protoxide of mercury, a white cloudiness ensues at the place where both fluids meet; disappearing, however, immediately on shaking, leaving the liquor as bright and transparent as before; without the chloride of sodium it would have retained its cloudiness. This deportment lasts until the nitrate of protoxide added exactly suffices to transform the chloride of sodium into corrosive sublimate; beyond this point a single drop of the mercurial salt produces a lasting white turbidity ($\text{HgO.NO}_3 + \text{Na.Cl} = \text{Hg.Cl} + \text{NaO.NO}_3$). If we know the amount of mercury in the solution of nitrate of protoxide of mercury, which has been added to a solution of urea containing an unknown quantity of chloride of sodium, until a permanent precipitate is formed, we consequently know the amount of chlorine or chloride of sodium present in the solution; one equivalent of mercury in the mercurial solution used, corresponding exactly to one equivalent of chlorine or chloride of sodium. Conversely, if the amount of chloride of sodium in the solution of urea be known, the amount of mercury in the mercurial solution that is consumed may be calculated.

2. On mixing solutions of nitrate of protoxide of mercury and common phosphate of soda, a white precipitate of phosphate of protoxide of mercury is at once formed, which soon becomes crystalline; a solution of corrosive sublimate may, however, be mixed with the alkaline phosphate without any turbidity. If we treat the fluid in which the phosphate of protoxide of mercury is deposited with chloride of sodium, before the precipitate has become crystalline, it re-dissolves, being decomposed with the chloride of sodium into corrosive sublimate and phosphate of soda. One equivalent of phosphate of protoxide of mercury requires to re-dissolve it one equivalent of chloride of sodium; if, therefore, the amount of chloride of sodium added be known, the amount of mercury in the mercurial solution may be calculated.

3. On gradually adding to a dilute solution of urea an equally dilute solution of nitrate of protoxide of mercury, and neutralizing the free acid of the mixture from time to time by baryta water or a dilute solution of carbonate of soda, a flocculent snow-white precipitate is obtained, which is quite insoluble in water. If the addition of the salt of mercury and of carbonate of soda be continued alternately as long as this precipitate is formed, a point is reached at which the addition of a drop of carbonate of soda occasions a yellow colour, from the formation of the hydrated oxide or the basic nitrate of mercury. All the urea is

⁵⁸ *Traité de Chimie Anat.*, vol. ii. p. 489.

⁵⁹ *Ann. der Chem. und Pharm.*, vol. lxxx. p. 114.

⁶⁰ *Ibid.*, vol. lxxxv. pp. 289—328.

⁶¹ *Der Harnstoff als Maass des Stoffwechsels*. Giessen, 1853.

⁶² We the less regret this omission, because the entire memoir is translated in vol. vi. of the *Quarterly Journal of the Chemical Society*.

then precipitated, and the precipitate contains four equivalents of protoxide of mercury to one equivalent of urea. Hence, if the quantity of mercury in the mercurial solution be known, we may calculate the amount of urea in the fluid, by observing the quantity of mercurial solution required for the complete precipitation. (We shall presently show that the occurrence of *allantoine* might interfere with the correctness of our results in determining urea quantitatively by this method.)

The following remarks, from the new edition of Lehmann's 'Physiological Chemistry,' will sufficiently explain the general steps of the process:—

"Liebig⁶³ has suggested a very ingenious method for determining volumetrically the amount of urea in the urine, which is closely connected with a chemical fact which he has recently discovered⁶⁴—namely, that if chloride of mercury (corrosive sublimate) in solution, and bicarbonate of potash in excess, be added to a solution

of urea, we obtain a compound of urea and mercury, $U + 4 HgO$, which is perfectly insoluble in water. This method has, further, this advantage, that we simultaneously determine the amount of *chlorine* in the urine. The following are the main steps in the process. In order to remove the phosphates and sulphates of the urine, a definite quantity of the fluid is mixed with half its volume of a fluid containing one volume of a saturated solution of nitrate of baryta to two volumes of a saturated solution of caustic baryta. We take about 15 c. c. of the filtered alkaline fluid (which consequently contains for every three volumes, two volumes of urine), and then, without neutralizing it, we add from a burette a solution of nitrate of protoxide of mercury of known strength, as long as any precipitate is formed. The mixture must be well stirred during this process. The precipitate is the above-mentioned compound of urea and protoxide of mercury

$[U + 4 HgO]$. When a few drops of the turbid fluid are poured into a watch-glass, and one drop of a solution of carbonate of soda is added, the mixture soon becomes yellow when treated with an excess of the solution of mercury, but it remains white when the solution of mercury is insufficient to precipitate all the urea. Very different methods may, of course, be employed for the preparation of the test-fluid (of nitrate of protoxide of mercury); Liebig has, however, proposed a very simple method for this purpose, which depends upon the fact that nitrate of the protoxide is decomposed by phosphate of soda, but that chloride of mercury (corrosive sublimate) is not thus affected. If, however, a solution of common salt, of known concentration, be added to a mixture of these salts before the precipitate of the phosphate of mercury has become crystalline, the quantity of the oxide of mercury may be very easily calculated from the volume of the chloride of sodium necessary for its re-solution (for one equivalent of chloride of sodium necessarily corresponds to one equivalent of the phosphate of mercury). We may, however, at once obtain a solution of chloride of sodium suited for the purposes of these experiments, when we consider that a solution which is saturated between the temperatures of 0° and 100° C. constantly contains 27% of salt.

"The method of determining the amount of *chlorine* in the urine is based upon the fact that, on the one hand, urea may be precipitated by the nitrate or protoxide, but not by the chloride of mercury; and, on the other hand, that the nitrate becomes converted into chloride of mercury when brought in contact with chloride of sodium. In order, therefore, to find the amount of chlorine in the urine, a definite volume of it should be decomposed with the solution of baryta; the urine which is filtered from the precipitate should then be treated with nitric acid until it is completely neutralized, and the solution of the nitrate of mercury poured upon it until the precipitate no longer dissolves on being stirred (that is to say, as long as chloride of mercury is formed). The quantity of the chloride of mercury, or of the chlorine, contained in the urine may be calculated from the volume of the solution of mercury which has been used."

⁶³ Ann. der Chem. und Pharm., vol. lxxxv. p. 289—298.
30—xv.

⁶⁴ Ibid., vol. lxxx. p. 128.

Liebig investigated the influence of the putrefaction of urine on this method of determining the urea, and has frequently obtained the same results with putrid urine, provided the decomposition had not progressed too far, as with fresh urine. As, however, in medical chemistry we always obtain fresh urine for examination, it is unnecessary for us to follow him in this branch of his inquiry.

In concluding this description of Liebig's celebrated method, it may be remarked, that Dr. Limpricht⁶⁵ has found that allantoine is likewise precipitated by nitrate of protoxide of mercury, in precisely the same manner as urea. Hence, if this substance were present in urine, it would be a source of error. Allantoine has, however, never yet been discovered either in normal or morbid human urine (even after the ingestion of uric acid⁶⁶), if we except the observations of Frerichs and Städeler,⁶⁷ who have found it in the urine of dogs when the respiration was impeded. (See p. 543.)

Neubauer⁶⁸ has recommended a modification of Millon's method, which consists in the decomposition of urea, by means of hyponitrous acid (NO_2HO), into carbonic acid, nitrogen and water, and the weighing of the carbonic acid. Neubauer induces this decomposition in an apparatus adapted to the quantitative determination of the carbonic acid, and determines the loss of weight arising from the nitrogen and carbonic acid, which are dried over chloride of calcium or sulphuric acid. On multiplying the loss of weight by 0.831,⁶⁹ we obtain the quantity of urea. Neubauer has satisfied himself by experiments that the accuracy of the result is unaffected by the extractive matters, uric or hippuric acid, or sugar. As Neubauer does not recommend, or even allude to, this method in his recently published work 'On the Analysis of the Urine,' we may conclude that he prefers Liebig's method, which he describes very fully.

Draper⁷⁰ determines the urea in urine from the quantity of carbonate of baryta which is formed when we gradually add nitroso-nitric acid to urine which has been freed by filtration from mucus and placed in a flask, through whose cork there passes a small funnel-tube, and a second tube to convey away the carbonic acid which arises. With the aid of an aspirator, the gas which is developed is caused to pass through two flasks containing baryta water. It takes about two hours to make one determination in this way. For further details, we must refer to the original memoir.

Dr. Edmund Davy⁷¹ has likewise published "A New Method of Determining the Amount of Urea," which has been already sufficiently noticed in this journal.⁷²

Dr. W. Marec has recently pointed out a method by which we may directly obtain the urea of the urine—that is to say, without making it pass through any combinations or decompositions. Full particulars of his mode of proceeding will be found in vol. ii. p. 512 of Robin and Verceil's 'Traité de Chimie Anatomie,' and (in his review of that work) in vol. xi. p. 375 of this journal. He arrives at the conclusion (which, as far as we are aware, has not recently been doubted either by physiologists or chemists), that "by far the largest proportion of this substance exists in the free state, as one of the immediate principles of human urine."

We postpone, till we treat of the *urine*, any consideration of the observations of Bischoff and others who have recently attempted to estimate the amount of this substance which is daily excreted. In connexion with the physiological relations of urea, we have only to observe, in the present place, that Verceil and Dollfus⁷³ have found it in the blood of cattle, and that Moleschott⁷⁴ detected it, in combination with oxalic acid, and associated with other oxalates, in the muscular

⁶⁵ Ann. der Chem. und Pharm., vol. lxxxvii. p. 99.

⁶⁶ Ibid., vol. lxxv. p. 340.

⁶⁷ Müller's Arch. für 1854, pp. 382 and 394.

⁶⁸ Quoted in Liebig and Kopp's Jahresbericht für 1853, p. 702.

⁶⁹ According to Gorup-Besanez, Neubauer's calculation is incorrect, and the following should be the rule. Multiply the loss of weight by 60, and divide the product by 100; the quotient is the quantity of urea. See Anleitung zur Zoochem. Analyse, p. 310, note.

⁷⁰ Phil. Mag., fourth series, vol. vi. p. 290.

⁷¹ Phil. Mag.

⁷² Vol. xiv. p. 531.

⁷³ Ann. der Chem. und Pharm., vol. lxxiv. p. 214.

⁷⁴ Arch. für Phys. Heilk., vol. xi. p. 493.

juice of frogs whose livers had been extirpated some days previously (Grohé,⁷⁶ however, who has repeated these experiments under Liebig's superintendence, could find neither urea nor oxalic acid in this fluid); that Hantz⁷⁶ found about 0.5% of urea (but no uric acid) in the alkaline, almost colourless, urine of toads; and that Fiedler⁷⁷ has confirmed the observations of Schottin and others regarding its occasional presence in the sweat.

Allantoine.—In connexion with this substance, it is sufficient for us to mention Limpricht's⁷⁸ memoir (to which we have already referred), "On the Combinations of Protoxide of Mercury with Allantoine;" a brief note by Wohler⁷⁹ "On the Fermentation of Allantoine," in which he mentions that a solution of this substance treated with yeast, and allowed to stand for four days at a temperature of 82° Fahr., ceases to contain allantoine, but contains in its place urea and the ammonia-salts of oxalic, carbonic, and some other syrupy acids; and a memoir of Frerichs and Städeler⁸⁰ "On the Occurrence of Allantoine in Urine in Cases of Impeded Respiration;" they found this substance in the urine of two dogs, the action of whose lungs was artificially impeded; and there was a doubtful trace in the case of a man with disease of the respiratory organs.

Taurine must now be added to the list of organic bases which can be formed artificially. Strecker⁸¹ has succeeded in forming it from isethionate of ammonia, $\text{NH}_4\text{O C}_4\text{H}_5\text{O}_2\text{SO}_3$, which $= \text{C}_4\text{H}_7\text{NO}_6\text{S}_2 + 2\text{H}_2\text{O}$, and therefore only differs from taurine by two equivalents of water. For his mode of proceeding, we must refer to the original memoir, or to vol. iii. p. 162 of the translation of Lehmann's 'Physiological Chemistry.'

FATS AND LIPIDS.

Heintz,⁸² who has for many years been actively engaged in the examination of the fats, has recently investigated the composition of mutton and beef suet, human fat, spermaceti, and the non-volatile fatty acids contained in butter. We may refer those of our readers who wish for the latest and most complete history of the chemistry of this class of bodies to Heintz's 'Zoochemie,' pp. 523—523, and pp. 1067—1080. In a memoir "On the Composition of Human Fat," published in vol. lxxxiv. of 'Pogg. Annalen,' and translated in an abridged form, in the fifth volume of the 'Quarterly Journal of the Chemical Society,' Heintz gives the following as the main results of his investigation:

1. Human fat does not consist merely of olein and margarin, but is a mixture of at least six different fats.
2. The first of these fats is present in very small quantity only, and appears to be identical with the *stearophantu* discovered by Francis in the berries of *Cocculus Indicus*. The formula of its fatty acid is $\text{C}_{36}\text{H}_{36}\text{O}_4$.
3. The second fat is a new substance, to which he gives the name of *anthropin*; its fatty acid crystallizes readily, and its formula is $\text{C}_{34}\text{H}_{32}\text{O}_4$.
4. The third fat is *margarin*.
5. The fourth fat is *palmetin*, which yields palmetic acid by saponification: it appears to be the most abundant of the four.
6. The liquid portion of human fat is composed essentially of *olein*, but it likewise contains a small quantity of *another fat*, which, by saponification, yields an acid whose baryta salt differs essentially, both in its physical and chemical characters, from oleate of baryta.

These views are materially modified in a subsequent memoir in vol. lxxxvii. of 'Pogg. Annalen,' in which he believes that he has shown:

⁷⁶ Ann. der Chem. und Pharm., vol. lxxxv. p. 245.

⁷⁷ Schmidt's Jahrbücher, 1854, vol. lxxxiv. p. 11.

⁷⁸ Ann. der Chem. und Pharm., vol. lxxxviii. p. 94.

⁷⁹ Müller's Arch. für 1854, No. 4.

⁸⁰ Pogg. Ann., vol. lxxxiv. p. 246; vol. lxxxvii. pp. 21, 267, 583; vol. lxxxix. p. 579;

vol. xc. p. 137.

⁸¹ Ibid., vol. lxxxiv. p. 127.

⁸² Ibid., p. 100.

⁸³ Comptes Rendus, vol. xxix. p. 61.

1. That *anthropic acid* is a mixture of about seven parts of palmitic acid with five of stearic acid.
2. That *margaric acid* is a mixture of about ten parts of palmitic acid with one part of stearic acid.
3. That the solid part of human fat consists only of *two* fats—namely, of stearin and palmitin, in which the palmitin strongly predominates.

Stearin has been carefully examined by Duffy.⁸³

The *fat of the elephant* has been investigated by Filhol and Joly;⁸⁴ the *oil of the squallus maximus* by Ronalds;⁸⁵ and the *fat of cantharides* by Gössmann.⁸⁶

We shall notice v. Bibra's observations on the brain-fats when treating of the chemistry of nervous tissue.

In connexion with the occurrence of fat in certain organs, both in health and disease, we may refer to the observations of Lang,⁸⁷ who found from 1·8 to 3·9% of fat in the dried substance of the kidneys of cats, but was unable to detect it in the kidneys of the ox or calf; and the observations of Lehmann⁸⁸ on the occasional appearance of fat in the kidneys of deer and hares. In human kidneys, fat was as often present as absent, according to Lang. Professor Beale's investigations, in reference to the amount of fat in the human liver and kidneys, have been already published in this journal.⁸⁹ Dr. Bacon⁹⁰ found that the fat in a very fatty liver amounted to 53·13 per cent.

We have not had an opportunity of consulting a prize thesis by Schultze⁹¹ bearing upon this subject.

The question regarding the *formation of fat* in the animal economy from starch, has been most decisively confirmed (if, indeed, any confirmation were required) by the observations of MM. Lacaze-Duthiers and Riche⁹² on the insects inhabiting galls. They have examined the composition of the galls, and of the larvæ of the cynips inhabiting them, and have incontestably proved that the fat which abounds in these larvæ is produced from the starch which forms the interior of the gall in which the animal lives.

NON-NITROGENOUS NEUTRAL BODIES.

Glucose, or Grape-sugar.—Horsley⁹³ has lately proposed an alkaline solution of bichromate of potash as a test for sugar. Many of the fallacies that may exist in the application of the reduction of copper test are pointed out by Dr. Lionel Beale, in his article 'On Sugar in Urine, and its Tests,' published in this Review.⁹⁴ Frerichs and Städeler, in the memoir to which we have already alluded (see p. 543), found that allantoine reduced oxide of copper, but that creatine had no such power.

In reference to testing for sugar, we may also refer to p. 467 of the last volume of the translation of Lehmann's 'Physiological Chemistry' (Lehmann there points out the cautions that must be adopted in order to obtain correct results with Fehling's method for determining sugar quantitatively), and to a memoir by Rigaud⁹⁵ on milk-sugar, grape-sugar, and the sugar of *quercus tinctoria*, in their relation to an alkaline solution of sulphate of copper.

It is now a definitely established fact, that sugar is a normal constituent of the blood.⁹⁶ V. Becker⁹⁷ has distinctly shown, by direct experiments, that its

⁸³ Quarterly Journal of the Chemical Society, vol. v. pp. 197, 303.

⁸⁴ C6. votes Rendus, vol. xxxv. p. 373.

⁸⁵ Chemical Gazette, vol. x. p. 420. 1852.

⁸⁶ Ann. der Chem. und Pharm., vol. lxxxvi. p. 317, and vol. lxxxix. p. 123.

⁸⁷ De Adipe in Urina et Renibus. Diss. Inaug. Dorpat, 1852.

⁸⁸ Lehrbuch der Phys. Chem., vol. i. p. 236.

⁸⁹ British and Foreign Medico-Chirurgical Review, vol. xii. p. 226.

⁹⁰ Quoted in British and Foreign Medico-Chirurgical Review, vol. xiv. p. 529.

⁹¹ De Adipis Genesi Pathologica: Comm. præmio orn. Gryphicæ. 1852.

⁹² Ann. des Sciences Natur., quatrième série, vol. ii. p. 81.

⁹³ Medical Times, vol. ix. p. 68. 1854.

⁹⁴ Vol. xi. pp. 106—116.

⁹⁵ Ann. der Chem. und Pharm., vol. xc. p. 297.

⁹⁶ See translation of Lehmann, vol. iii. pp. 467—529.

⁹⁷ Zeitschr. für Wissensch. Zoologie, vol. v. p. 123. 1853.

amount is influenced by highly saccharine food. Thus, for instance, he found that the blood of rabbits which had been fed solely on carrots yielded 0.584% of sugar, while there was only 0.109% in the blood of those animals when fed upon oats, and only 0.045% when they had fasted for twenty-four hours. As much as 1.198% of sugar was found in the blood of a rabbit which had been so abundantly supplied with sugar, from time to time, during several hours, that some of this substance had even passed into the solid excrements.

Sugar has been detected by Frerichs⁹⁸ in the *liquid of ascites*. The case was that of a girl, aged nine years, with lardaceous liver. The liquid, removed in the ordinary manner (by tapping), was rich in sugar, a substance hitherto not found in this situation. He has since often attempted to discover it in similar cases proceeding both from hepatic and cardiac disease, but without success. Dr. G. D. Gibb⁹⁹ has likewise found sugar in the fluid of ascites.

The question regarding the *formation of sugar in the liver* is one of so much physiological interest, that we shall notice the leading investigations bearing on this subject, although one or two of them hardly fall within the limits of time to which this Report is devoted. Bernard and Barreswil¹⁰⁰ have found sugar in the hepatic tissue even of animals not using saccharine or amylaceous food. Bernard¹⁰¹ subsequently extended his researches, and showed the existence of sugar in the liver, not only of all vertebrates (so far as he experimented on them), but also in that of the Gastropoda, Acephala, and Decapoda. Frerichs¹⁰² has confirmed these observations for the liver of man and many animals; Van der Broek for that of dogs and rabbits; Baumert¹⁰³ for that of the fox, the dog, the cat, and the sheep; and Lehmann and Kunde¹⁰⁴ for that of frogs. Vernois¹⁰⁵ in a very elaborate memoir "On the Sugar of the Liver, and the Modifications which it undergoes in Disease," has subsequently contributed materially to our knowledge of this subject. A brief abstract of his researches is given in vol. xii. p. 550 of this Review. We must also direct the attention of our readers to a short but very valuable article by Valentin¹⁰⁶ "On the Existence of Sugar in the Liver and other parts of Hibernating Animals," to Dr. Pavcy's¹⁰⁷ memoir "On Saccharine Matter and its Physiological Relations to the Animal Economy," and to v. Becker¹⁰⁸ "On the Behaviour of Sugar in the Animal Metamorphosis." (Several references to v. Becker's most important memoir will be found in future parts of this Report.)

Moleschott¹⁰⁹ operated upon the livers of twelve frogs, and found undoubted evidence of sugar by Trommer's test. The question arises, Is this sugar of the liver derived from the blood, or is it formed by the actual hepatic tissue? Bernard advocates the latter view, since he has thus obtained sugar, wholly independent of food, from carnivorous and herbivorous animals, from animals during hibernation, and from the fetus. These observations have been confirmed by Frerichs, Van der Broek, Baumert. Further, both Bernard and Lehmann have found that the portal blood of the dog and horse contains little or no sugar, while the blood of the hepatic veins differs from that of any other venous blood, in containing this substance in considerable quantity. To these data, Moleschott adds an important fact. "If," he observes, "the sugar is not formed in the liver, but only strained off by it from the blood, then the blood of those animals whose liver had been removed, would be found recharged with sugar, exactly as uræa accumulates in the blood when the kidneys have been extirpated; but with frogs, some of which had been without the liver for two and even three weeks, I found no

⁹⁸ Wien. Med. Wochenschr., No. 6, 1854. See this journal, No. 28, p. 416, foot-note.

⁹⁹ Med. Times, vol. iv. p. 451.

¹⁰⁰ Comptes Rendus, vol. xxvii. p. 514.

¹⁰¹ Ibid., vol. xxxi. p. 572.

¹⁰² Handwörterbuch der Physiologie, vol. iii. part 1, p. 831.

¹⁰³ Journ. für Pr. Chem., vol. liv. p. 357.

¹⁰⁴ Kunde: De Hepatis Ranarum Extirpatione. Diss. Berol., 1850.

¹⁰⁵ Arch. Gén. de Méd., cinquième série, vol. i. p. 667. 1853.

¹⁰⁶ British and Foreign Medico-Chirurgical Review, vol. xlii. p. 533. 1854.

¹⁰⁷ Guy's Hospital Reports, vol. viii. p. 319.

¹⁰⁸ Zeitsch. für Wissen. Zool., vol. v. p. 123. 1853.

¹⁰⁹ Müller's Archiv für 1853, p. 156.

sugar in the blood, flesh, gastric juice, urine, or, finally, in the water in which twenty-six of these animals, thus, mutilated, had passed two days." From these facts, Moleschott concludes that the sugar contained in the liver is formed by the liver itself.

Inosite, or muscle-sugar, has been further investigated by its discoverer, Scherer,¹¹⁰ who gives the following test for it:—If we evaporate inosite, or a mixture containing it, with nitric acid on platinum foil, almost to dryness, and then moisten the residue with ammonia and a little chloride of calcium, and again carefully evaporate to dryness, a vivid rose-red tint is exhibited on the platinum foil. Other carbo-hydrates, as milk-sugar, starch, cane-sugar, glucose, &c., yield no such reaction. The test is as sensitive as it is characteristic; half a milligramme (about $\frac{1}{16}$ th of a grain) yielding a very intense colour.

Socoloff¹¹¹ found that he could readily obtain inosite from the muscular structure of the heart, but not a trace of it from the juice of any other muscles; and Pannum of Copenhagen arrived at precisely similar results.

The occurrence of *cellulose* as a constituent of the animal body, has been so fully discussed by Dr. Arlidge in the last volume of this Review (pp. 439—448, and pp. 470—482), that it is unnecessary to do more than refer our readers to that gentleman's review and memoir.

PIGMENTS.

Heintz¹¹² has published a memoir "On the Pigments in Gall-Stones," containing the results of numerous ultimate analyses.

Virchow, as is well known, some years ago discovered peculiar reddish-yellow elongated crystals in the bile of persons who had suffered from cancer of the liver, or retention of the bile consequent on catarrh of the gall-bladder.¹¹³ To these crystals he assigned the name of *bilifulvin*; and he frequently pointed out that there is a great similarity between this bilifulvin and hamatoidin (a red or reddish-yellow substance, either crystalline or amorphous, discovered by Virchow in extravasated blood, and to which we shall presently refer more fully). Zenker and Funke have recently shown, that if these substances are not identical, there is, at all events, the closest relationship between them, since they have independently proved that bilifulvin may be very readily converted into hamatoidin.

The *urine pigments* in disease have been made the subject of investigation by several chemists. We can do little more than mention the titles of their memoirs. Virchow¹¹⁴ has described the *blue pigment* which occasionally occurs, and has given the results of his investigations in the memoirs referred to in the foot-note. Heller¹¹⁵ has published a memoir "On Uro-erythrin as a Constituent of the Urine in Disease;" Dr. Harley has published two memoirs, one entitled, "Researches on the Colouring Principles of the Urine,"¹¹⁶ and the other, "On Uro-hamatin and its Combination with Animal Resins."¹¹⁷ Kletzinsky¹¹⁸ has contributed a paper "On Uro-glucanin considered as an Oxide of Indigo;" Hassall¹¹⁹ two memoirs "On the frequent Occurrence of Indigo in Human Urine, and on its Chemical, Physiological, and Pathological Relations;" and lastly, v. Sicherer,¹²⁰ a memoir "On the Formation of Indigo in the Human Organism."

¹¹⁰ *Ann. der Chem. und Pharm.*, vol. lxxxi. p. 375.

¹¹¹ *Ibid.*

¹¹² *Pogg. Anh.*, vol. lxxxiv. p. 196.

¹¹³ They are depleted in Funke's Atlas, Plate VI. figs. 2 and 4.

¹¹⁴ *Archiv für Path. Anat.*, vol. i. p. 423; *Verhandl. der Phys.-Med. Ges. zu Würzburg*, vol. ii. p. 308; *Arch. für Path. Anat.*, vol. iv. p. 316; and vol. vi. p. 259.

¹¹⁵ *Arch. für Chem. und Mikrosk.*, p. 361. 1853.

¹¹⁶ *Pharmaceutical Journal*. Nov. 1852.

¹¹⁷ *Verhandl. der Phys.-Med. Ges. zu Würzburg*, vol. v. p. 1.

¹¹⁸ *Archiv für Chem. und Mikrosk.*, p. 414. 1853.

¹¹⁹ *Proceedings of the Royal Society*, vol. vi. p. 327; and vol. vii. p. 122.

¹²⁰ *Ann. der Chem. und Pharm.*, vol. xc. p. 120.

The animal pigments are very fully considered by Heintz in his 'Zoochemie,' and by Verdeil and Robin, in the third volume of their 'Traité de Chimie Anatomique.'

THE PROTEIN BODIES.

Numerous memoirs have been published upon this subject, amongst which we may especially notice the following:

MIALHE:¹²¹ On Albumen, and the different States in which it occurs in the Animal Economy.

SCHERER:¹²² On Paralbumen and Metalbumen.

PANUM:¹²³ On Fibrin in general, and its Coagulation in particular.

PANUM:¹²⁴ New Observations on the Albuminous Bodies.

LIEBERKUHN:¹²⁵ On Albumen and Casein.

LIEBERKUHN:¹²⁶ On the Action of Acetic Acid and the Alkalies on Albumen.

PARKES:¹²⁷ On the Decomposition of Chloride of Sodium by Acetic Acid in the presence of Albumen, or the Coagulation of the Albumen of the Serum in the presence of Acetic Acid and a certain amount of Chloride of Sodium.

PARKES:¹²⁸ On the Precipitation of Albumen by Acids and Neutral Salts.

LECONTE and DE GOÛMOENS:¹²⁹ On the Albuminoids.

VIRCHOW:¹³⁰ On a peculiar Behaviour of Albuminous Fluids on the addition of Salts.

MÜLLHAUSEN:¹³¹ On certain Products of Decomposition of the so-called Protein Compounds.

We must content ourselves with referring the reader to the additions and notes to the translation of Lehmann's 'Physiological Chemistry,' for an abstract of the most important of the above memoirs.

There is one protein-body, however, to which we must allude more fully—namely, "the crystallizable protein substance of the blood," to which Lehmann has provisionally applied the term *Hæmato-crystalline*, and which was previously named *hæmatoidin* by Virchow.

The observations of Funke (1852), Kunde (1852), and Parkes (1852) on this subject are so fully given by Dr. Sieveking, in a critical review which recently appeared in this journal,¹³² that we need only notice the later investigations on this important subject. Lehmann has paid more attention to this substance than any other chemist; and an abstract of his views regarding it will be found in pages 485—495 of the third volume of the translation of his 'Physiological Chemistry.' He has, however, treated the subject more fully in three memoirs¹³³ in the 'Berichte der Gesellschaften der Wissenschaften zu Leipzig,' 1852.

In the latest of these memoirs he gives the ultimate analysis of hæmato-crystal-

¹²¹ L'Union Médicale, Nos. 80, 81, 83, 86, 90, 1852; and Schmidt's Jahrbücher, vol. lxxvi. p. 287. 1852.

¹²² Ann. der Chem. und Pharm., vol. lxxvii. p. 135. (Paralbumen has been recently found by C. O. Weber, in the contents of a tumour in a child nine weeks old. Arch. für Path. Anat., vol. vi. p. 520.)

¹²³ Schmidt's Jahrbücher, vol. lxxvi. p. 153. 1852. (An epitome of a Danish thesis.)

¹²⁴ Arch. für Path. Anat., vol. iv. p. 41, 1852; and Schmidt's Jahrbücher, vol. lxxv. p. 274. 1852.

¹²⁵ Pogg. Ann., vol. lxxvii. pp. 117, 296. 1852. (An abstract of this memoir is given in the Chemical Gazette, vol. x. p. 366. 1852.)

¹²⁶ Arch. für Path. Anat., vol. v. p. 162.

¹²⁷ Medical Times, new series, vol. i. p. 84. 1850.

¹²⁸ Medical Times and Gazette, vol. v. p. 6. 1852.

¹²⁹ Arch. Gén. de Méd., Juin, 1853; and British and Foreign Medico-Chirurgical Review, vol. xlii. p. 538.

¹³⁰ Arch. für Path. Anat., vol. vi. p. 572.

¹³¹ Ann. der Chem. und Pharm., vol. xc. p. 171.

¹³² British and Foreign Medico-Chirurgical Review, vol. xlii. p. 348. 1853.

¹³³ A good abstract of these memoirs is given in the Annalen der Chem. und Pharm., vol. lxxxviii. p. 377.

line obtained from the blood of the dog. After being extracted with alcohol, ether, and water, it yielded, after deducting the ash, the following numbers :

	I.	II.	III.
Carbon	55.41 ...	55.24 ...	55.18
Hydrogen	7.08 ...	7.12 ...	7.14
Nitrogen	17.27 ...	17.31 ...	17.40
Oxygen, with a little sulphur . .	20.24 ...	20.33 ...	20.28

The sulphur in three cases was determined at 0.253, 0.206, and 0.248 per cent.

We believe that the latest researches on this subject are those of Teichmann,¹³⁴ whose memoir "On the Crystallization of the Organic Constituents of the Blood," has been fully noticed in vol. xiii. p. 541 of this Review.

THE MINERAL SUBSTANCES OF THE ANIMAL BODY.

Burin du Buisson¹³⁵ has confirmed the statements of Millon and Hannon that *manganese* exists in human blood. The following is the best method of demonstrating its existence:—We treat the blood-ash with a mixture of nitric and nitrous acids, boil it, evaporate it, and re-dissolve it in water, till it becomes neutral; we then precipitate the iron with benzoate of ammonia, and afterwards the manganese with carbonate of soda. As a mean of ten experiments, he found that 1000 parts of blood contained 0.078 of peroxide of manganese. He further believes that he has found that the manganese belongs solely to the corpuscles, and not to the serum.

Wackenroder¹³⁶ has published an elaborate memoir "On the Presence of Small Quantities of Copper in the Animal Organism." The following are his conclusions :

1. Domestic animals which live upon pure vegetable food have no copper in their blood, or, at least, not sufficient to render it observable in half a pound of blood.

2. The blood of man and domestic animals living upon mixed food may contain very considerable quantities of copper, and sometimes also of lead. The presence of these metals is, however, by no means to be regarded as constant or normal.

3. The origin of these small quantities of copper and lead can only be attributed to nutritive or medicinal substances containing these metals.

4. It is not probable that these portions of copper and lead, small though they be, can remain without any influence on the human organism.

5. The bodies of many animals of the lower classes, snails for instance, invariably contain a proportionally large quantity of copper.

Considerable discussion has recently taken place regarding the presence of *iodine* in organized beings. Chatin a few years ago found it not only in all spring waters, in plants, and in most kinds of food and drink, but also as an essential constituent in land and fresh-water animals. Lohmeyer¹³⁷ has tested Chatin's statements with great care. Besides finding no iodine in the air (in which Chatin determined it *quantitatively*), he was unsuccessful in searching for it in cows' milk or in hens' eggs.

THE CHEMISTRY OF THE ANIMAL FLUIDS.

The Saliva.—The chemistry of this secretion was so fully discussed by the author of this Report on a recent occasion in this journal (vol. xii. pp. 167—178), that little remains to be noticed on the subject. The most important works and memoirs published since the beginning of 1852 on the saliva, are :

¹³⁴ *Zeitschr. für rat. Med.*, new series, vol. iii.

¹³⁵ *Revue Médicale*, Février, 1852.

¹³⁶ *Arch. der Pharm.*, vol. lxxv. pp. 140, 268; and vol. lxxvi. p. 1. (Quoted in the *Chemical Gazette*, vol. xli. p. 172. 1854.)

¹³⁷ *Nachrichten von der Gesellsch. der Wiss. zu Göttingen*, p. 181. 1853. (Quoted in the *Chemical Gazette*, vol. xi. p. 352. 1853.)

1. BIDDER AND SCHMIDT: *Verdaunungssäfte und Stoffwechsel*, pp. 1—28 (of which we have given a tolerably full abstract in the above-mentioned review).
2. COLIN:¹³⁸ *Experimental Researches on the Salivary Secretion in the Solidungulates and Ruminants*.
3. BECHER AND LUDWIG:¹³⁹ *On the Law which Regulates the Chemical Composition of the Submaxillary Saliva in the Dog*.
4. BERNARD:¹⁴⁰ *On Elective Elimination by the Salivary and other Secretions*.
5. BERNARD:¹⁴¹ *A Memoir on the Saliva*.
6. THORNTON HERAPATH:¹⁴² *On the Quantitative Determination of the Sulpho-cyanide of Potassium in Saliva*.

Colin finds that if the solidungulates the secretion of the two parotids alternates, the parotid of the side on which mastication is going on secreting at least one-third more than that of the opposite side. He did not observe this alternating action in the secretion of the submaxillary glands, which is apparently uniform on both sides. When the animal consumes dry food, there are secreted from 5000 to 6000 grammes of saliva from all the glands in the course of one hour; about one-third or one-fourth more when the animal consumes oats, and one-third or one-fourth less when living on succulent roots.

Becher and Ludwig found that the solid residue of the saliva diminishes in proportion to the amount which the gland has already yielded; the organic constituents sinking far more rapidly than the inorganic. Fluctuations in the quantity of water in the blood did not disturb this law, as was proved by the examination of saliva collected after one or more venesections; nor was it affected by the injection of chloride of sodium into the blood, although the quantity of the salts in the saliva was somewhat augmented thereby.

Bernard's memoir "*On Elective Elimination by the Salivary and other Secretions*," has been already noticed in this journal (vol. xi. p. 558).

Bernard's memoir "*On the Saliva*," although of considerable length, contains little or nothing that is worthy of being extracted. The organic substances occurring in mixed saliva, are (according to him) albumen, casein, epithelium, phosphorized fat, mucus, and a peculiar organic matter; and he denies the ordinary occurrence of sulpho-cyanide of potassium in the saliva.

The following is Herapath's method of determining the sulpho-cyanogen in saliva. The saliva was first evaporated to dryness in a water-bath or gas-oven, and the residue was acted upon with dilute hydrochloric acid. The solution, after being filtered, in order to remove any flocculi that might be suspended in it, was poured into a colorimeter, where it was diluted with distilled water to the necessary degree, care having been taken to add previously a drop or two of a solution of perchloride of iron. The test produced was then compared with that of a standard solution of pure sulpho-cyanide of ammonium, containing a known per-centage of sulpho-cyanogen. The following is an abstract of his results: In 10,000 parts of the saliva of a man aged 24 years, the mean of five experiments was 0.379 of sulpho-cyanogen (or 0.634 of sulpho-cyanide of potassium). In two of these experiments, where the saliva was obtained by smoking tobacco, the mean quantity of sulpho-cyanogen was 0.605; while, in the three cases in which it was not smoked, the mean quantity was only 0.288. In 10,000 parts of the saliva of a woman aged 28, the mean of three experiments was 0.104 (corresponding to 0.173 of sulpho-cyanide of potassium); the maximum being 0.2027, and the minimum 0.0186. Hence it is obvious that the amount of sulpho-cyanogen in the saliva, even of the same individual, is liable to great fluctuations.

The Gastric Juice has been made the subject of inquiry by the following chemists:

¹³⁸ *Comptes Rendus*, vol. xxxiv. pp. 681, 977.

¹³⁹ *Zeitsch. für rat. Med.*, new series, vol. i. p. 278.

¹⁴⁰ *Arch. Gén. de Méd.*, new series, vol. i. p. 5.

¹⁴¹ *Gazette Médicale*, Nos. 7, 11, 22, 23. 1853.

¹⁴² *Chemical Gazette*, vol. xi. p. 294. 1853.

1. BIDDER AND SCHMIDT: Die Verdauungssäfte und der Stoffwechsel, pp. 29—97. (Noticed fully in the review already mentioned.)
2. GRUENEWALDT: Succus Gastrici Humani Indoles Physica et Chemica, etc. 1853.
3. SCHROEDER: Succus Gastrici Humani Vis Digestiva, etc. 1853.

These theses by Gruenewaldt and Schroeder have been so fully noticed in p. 263 of this volume, that it is needless to discuss them in this, their more proper place. We will merely remark that the *sarcina* was frequently observed in the gastric juice examined by Gruenewaldt, both when the stomach was empty and when full, the woman with a gastric fistula on whom he experimented being apparently in perfect health. Hence he agrees with Virchow,¹⁴³ that this organism must not be regarded as a special symptom of a peculiar form of disease.

The *Bile* has not been submitted to chemical examination during the time to which this Report is devoted, but a very large amount of information on many points bearing on its chemistry and physiology, will be found in the work of Bidder and Schmidt, to which we have so often referred (pp. 98—239). (The most important of their results are given in the Appendix to the translation of Lehmann's 'Physiological Chemistry'.)

Moleschott¹⁴⁴ has recently instituted a series of carefully conducted experiments on frogs, with the view of determining the seat of the formation of the main constituents of the bile. Like Kunde,¹⁴⁵ he extirpated the liver, but succeeded in keeping the animals alive for a longer period. He could not succeed in detecting a trace of the biliary acids or of bile-pigment, either in the blood, or in the lymph, or in the flesh, or in the urine of the frogs on which he operated. Hence he regards it as an established fact, that the essential constituents of the bile are previously formed in the liver.

Enderlin,¹⁴⁶ however, believes that cholic acid (or cholate of soda) is a normal constituent of the blood. He is led to this view from observations made with Pettenkofer's test on the alcoholic extract of the dried blood of a pregnant woman, and on that of animals. Some of Enderlin's previous results have been completely overthrown by subsequent inquiry, and such will probably be the fate of this.

Planta and Kekulé¹⁴⁷ have published analyses of two gall-stones found in the gall-bladder of a man aged 60, who committed suicide; and Wolff¹⁴⁸ has analyzed a concretion found in the gall-bladder of a woman, aged 29, who died from tuberculosis.

The *Pancreatic Juice* has been examined by the following chemists:

1. BIDDER AND SCHMIDT: Die Verdauungssäfte und der Stoffwechsel. pp. 240—259.
2. COLIN.¹⁴⁹ On the Pancreatic Secretion of the larger Domestic Ruminants.
3. WEINMANN. Ueber die Secretion der Bauchspeicheldrüse. Dissertation inaug. 1852.
4. KROEGER: De Succo Pancreatico. Dissertation inaug. 1854.

The quantity of the pancreatic juice varies very much in different animals. According to Colin, it does not stand in a direct ratio to the volume of the gland. While the pancreas of the ox and of the horse yields 260 or 270 grammes in an hour, that of the swine, which is about half the size, yields only 12 or 15 grammes in an hour. Bidder and Schmidt found that a strong dog, weighing 20 kilogrammes (the kilogramme is about 2·2 lbs. avoirdupois), secreted 7·86 grammes

¹⁴³ Arch. für Path. Anat., vol. i. p. 268.

¹⁴⁴ Arch. für Physiol. Heilkunde, vol. xi. p. 479.

¹⁴⁵ Diss. Inaug. Berol. 1850.

¹⁴⁶ New York Mon.-Schr., lii. 1852. (Quoted in Schmidt's Jahrbücher, vol. lxxvii. p. 4. 1853.)

¹⁴⁷ Ann. der Chem. und Pharm., vol. lxxxvii. p. 367.

¹⁴⁸ Quoted in Liebig and Kopp's Jahresbericht für 1853, p. 616.

¹⁴⁹ Comptes Rendus, vol. xxxii. p. 374, and vol. xxxiii. p. 85.

(the gramme being about 15·4 grains) in eight hours and a quarter; there being 1·614 grammes secreted in the first hour, while in the eighth there was only 0·73 of a gramme. In this case, however, the secretion was only collected from the lower and larger duct, while the course of the fluid into the intestine through the upper and smaller one was not impeded. From these observations on the dog, Bidder and Schmidt calculate that an adult man, weighing 64 kilogrammes (or about 10 stone) secretes 150 grammes (or nearly 4·8 ounces) in twenty-four hours. The experiments of Kroeger, which were also made in conjunction with Bidder, yield a far higher result. He finds that a dog, for every kilogramme's weight, secretes, on an average, 89·3 grammes of pancreatic juice in twenty-four hours; and hence he calculates that an adult man, weighing 64 kilogrammes, would secrete, in twenty-four hours, 5·715 kilogrammes, or 12·6 lbs.—that is to say, about the eleventh part of the weight of the whole body. Weinmann, who was assisted by Ludwig, found in a series of thirty-seven observations, extending over seven days, that a dog, for every kilogramme's weight, secretes 35·184 grammes in twenty-four hours; hence calculating from these data, a man would secrete 2·252 kilogrammes (or nearly 5 lbs.) daily. As the views of Bidder and Schmidt are pretty fully given in "The Chemistry of Digestion," in the twelfth volume of this journal, and likewise in the Appendix to the translation of Lehmann's 'Physical Chemistry,' and as an abstract of Kroeger's thesis may be found in the last number of this journal, it is unnecessary to pursue the subject further in the present place.

The *Intestinal Juice* has not, so far as we are aware, been subjected to examination since the publication of Bidder and Schmidt's work. A full report of their investigations is given in "The Chemistry of Digestion," and in the Appendix to Lehmann.

The *Intestinal Gases* have been carefully examined by Valentin,¹⁶⁰ in a memoir entitled, "Remarks on the Gases of Digestion of Horses." An abstract of his researches is given in page 265 of the present volume.

The *Excrements* have been made the subject of inquiry by the following investigators:

1. OSBORNE.¹⁵¹ The Examination of the Fæces in Disease.
2. WEHSARG: Mikroskopische und chemische Untersuchungen der Fæces gesunder erwachsener Menschen. Inaug. Abhandl. Giessen, 1853.
3. LIRING: Mikroskopisch-chemische Untersuchungen menschlicher Fæces unter verschiedenen pathologischen Verhältnisse. Inaug. Abhandl. Giessen, 1853.
4. MARCET:¹⁵² On the immediate Principles of the Excrements of Man and Animals in the Healthy Condition.

Osborne's is rather a medical than a chemical paper; and the theses of Wehsarg and Liring have been noticed in vol. xiv. p. 528 of this journal. It appears from Marcet's experiments, that healthy human excrements contain:

1. A new organic substance, possessing an alkaline reaction, which its discoverer names *ecretine*. In its pure state it appears in circular groups of crystals, which have the form of acicular four-sided prisms, and polarize light very readily. It is very soluble in ether, cold or hot, but sparingly soluble in gold alcohol; it is insoluble in water, and is not decomposed by dilute mineral acids. It fuses between 200° and 203° Fahr., and at a higher temperature burns away with inorganic residue. It does not dissolve when boiled with a solution of potash. It contains nitrogen and sulphur, though in small proportions. The products of its

¹⁵⁰ Arch. für Physiol. Heilkunde, vol. xiii. p. 356. 1854.

¹⁵¹ Dublin Quarterly Journal of Medical Science, No. 29, February, 1853, p. 104.

¹⁵² Proceedings of the Royal Society, June 15th, 1854, vol. vii. p. 148.

decomposition have not yet been investigated. Marcet considers that it exists for the most part in a free state in the excrements, and constitutes one of their immediate principles. As to its source, he observes that it appeared in excess when a considerable quantity of beef had been taken, and in less than the usual quantity in a case of diarrhoea, attended with loss of appetite; but none could be directly obtained from beef on subjecting it to the same process of extraction as faeces; neither could it be found in ox-bile, the urine; or the substance of the spleen.

2. A fatty acid, having the properties of margaric acid, but not constantly present. He is uncertain whether the margaric acid in the faeces is free, or combined with excretine; but he is disposed to conclude that the neutral fats are decomposed in the intestinal canal, and their acid set free. Not having been able to discover stearic acid in human evacuations, he supposes that what is contained in the fat taken in the food must be converted into margaric acid in its passage through the alimentary canal.

3. A colouring matter similar to that of blood and urine.

4. A light granular substance, which he is inclined to regard as a combination of phosphate of potash and a pure organic matter.

5. An acid olive-coloured substance, of a fatty nature, which he names *excretolic acid*. It fuses between 77° and 79° Fahr., and at a higher temperature burns without residue. It is insoluble in water and in a boiling solution of potash, is very soluble in ether and in hot alcohol, and slightly so in cold water. He believes that it is combined in the excrements in the form of a salt with excretine or a basic substance closely allied to it.

6. No evidence of butyric or of lactic acid was obtained.

The faeces of various animals yielded the following results:

1. The excrements of carnivorous mammals—viz., the tiger, leopard, and dog (fed on meat)—contain a substance allied in its nature to excretine, but not identical with it. They contain no excretine, but yield butyric acid, which is not present in human excrements.
2. The excrements of the crocodile contain cholesterin, and no uric acid, while those of the boa yield uric acid, and no cholesterin. [It is probable that the semi-solid urine and the excrements were not duly separated in this experiment.—*Rep.*]
3. The faeces of herbivorous animals—viz., the horse, sheep, dog (fed on bread), wild boar, elephant, deer, and monkey—contain no excretine, no butyric acid, and no cholesterin.

A memoir by Zimmermann,¹⁵³ entitled "A Contribution to the Knowledge of Typhus-stools," belongs rather to microscopy than chemistry.

(To be concluded in the next number.)

¹⁵³ Deutsche Klin., 19, 1854; and Schmidt's Jahrbücher, vol. lxxviii. p. 147. 1854.

ANNALS OF MICROLOGY.*

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[Third Year.]

PART I.—ANATOMICAL AND PHYSIOLOGICAL MICROLOGY.

THE notices of investigations in this department which we have reviewed in the present part of our Annals will be found to be less numerous, and in several respects less important, than those which we have had to present to our readers on former occasions. The chief labours of microscopic inquirers appear to have been within the last year devoted to pathological histology. In some quarters important memoirs have appeared relating to comparative or zoological histology (as those of Leydig, Müller's Archiv); but to notice these would lead us too far from the more immediate object of these Annals, which is the elucidation of those branches of histology interesting to the medical observer. It is, doubtless, hardly consistent with science to draw the line which separates comparative from human histology, so much do these subjects illustrate each other; but, for practical purposes, the distinction is necessary. In the compilation of these Annals we have to acknowledge ourselves especially indebted to that ample record of contemporary medical science, Schmidt's Jahrbücher.

HISTO-GENESIS.

Origin of Epithelial Tissue.—Gunsburg,† from the result of investigations on embryos of from five to six weeks to three months, regards this structure as formed by the solidification of the plasma into a textureless basis-membrane, in which subsequently elementary granules appear; around these granules envelopes are formed, and afterwards a nucleus becomes developed in the centre of the cell-vesicle. This

* Owing to the departure of Mr. Holmes Coote for Smyrna, the Surgical Report is omitted from this number. The Report on Forensic Medicine is postponed for want of space. The following Foreign Journals were received during the months of December, January, and February:

GERMAN.

1. Archiv für Anatomie, &c., von J. Müller. Dec.
2. Archiv für Pathol. Anat., von Virchow. Band vii. Heft 2.
3. Archiv für Phys. Heilkunde, von Vierordt. 1854, Heft 4.
4. Zeitschrift von Rat. Med., von Henle und Pfeiffer. Band v. Heft 1, 2.
5. Zeitschrift der K. K. Gesell. der Aertze zu Wien, von Hebra. Nov., Dec., Jan.
6. Zeitschrift für Klinische Med., von Gunsberg. Band vi. Heft 1.
7. Zeitschrift (Henke's) für die Staatsarzneikunde, von Behrend. 1855, Heft 1.
8. Annalen des Berlin Charité-Krankenhaus. Band v. Heft 2.
9. Schmidt's Jahrbücher. Dec., Jan., Feb.
10. Verhandlungen der Phys.-Med. Gesell. zu Würzburg. Band v. Heft 3.
11. Vierteljahrsschrift für die Prak. Heilk. Prag, 1854. Band 4.

FRENCH.

12. Revue Méd. Chir. de Paris. Dec., Jan., Feb.
13. Bull. Gén. de Thérapeutique. Dec., Jan., Feb.
14. Archives Générales de Médecine. Dec., Jan. Feb.

ITALIAN.

15. Bulletino delle Scienze Mediche. Bologna. August, Sept.

NORWEGIAN.

16. Norsk Magazin for Lægevidenskab. Bind viii. Hefte 5—10.

EAST INDIAN.

17. Indian Annals. No. 3.

AMERICAN.

18. American Journal of the Medical Sciences.
19. Philadelphia Medical Examiner.

† Schmidt's Jahrbücher, No. 2, p. 149. 1855.

view, if confirmed, would obviously assign to the nucleus a position of very little importance, and indeed it would then be almost impossible to conceive what physiological value it possessed.

This author has followed out the development of the cylindrical epithelium in the lungs and intestinal canal in three embryos of six weeks, eight weeks, and five months respectively, and concludes that it takes place exactly in the same way as in the epidermis.

Blood and Bloodvessels.—Some essays of Drummond* on these subjects may be consulted with advantage.

Hairs.—A memoir on the origin, nature, and growth of hairs, by Reissner,† will tend to confirm the views of other recent observers.

PERMANENT TISSUES AND DEFINITE ELEMENTS.

So much importance attaches to the determination of the primary physiological and anatomical characters of so extensive an element as the connective tissue, and so novel are the views recently advanced with regard to it by Virchow, that we think it necessary to give, somewhat at length, an account of the researches of Bruck on the same structure, which differ considerably from those of Virchow.

Connective Tissue.—Bruck (Prof. C. of Basle) has investigated the connective tissue, at various stages in the progress of its development. He refers its origin to the fibrillation, or splitting into fibres, of a dense structureless blastema, by a process of simple dehiscence, without any intervention of cells, into a fibrous form of intercellular substance; but he does not claim this mode of genesis for all the structures to which the name of connective tissue is applied. In the tissue of the umbilical cord, and that of the Whartonian structure, it can be shown that the gelatinous tissue of Schwann does not disappear, but progressively increases, separates the cell-elements from each other, and finally exhibits a clear striation, splitting, and fibrillation in the longitudinal axis of the cord. There is no evidence of a cell-metamorphosis contributing to this formation of fibres. In other parts of the body, the direct origin of the fibre-tissue, by dehiscence of a structureless intercellular substance, is more difficult to be shown, though the author thinks he has often witnessed a secondary increase of the so-called formed intercellular substance. Other tissues, however, are found which are undoubtedly developed, not from intercellular substance, but from the melting together or fusion of cells; thus, the amnion and allantois, according to Bruck, manifestly show originally a cellular formation; but the contours of the polyhedral cell disappear very early, though their nuclei remain longer, often become elongated, and, in the further growth of the membrane, becoming separated from each other, appear less numerous than at first.

^o In this perfectly homogeneous, transparent, and very thin membrane, fine folds appear, in the direction of which it may be torn artificially into stria and fibres. The same may be observed in the umbilical vesicle in animals in which it continues longer. In all these cases there is to be seen, in a secondary membranous blastema, a tendency to the formation of folds, splittings, and fibrillations. The same takes place equally, more or less, in all investing tissues—as the capsules of the Pacinian bodies, the membrana propria of glands, and the sheaths of the primitive muscular fibres. To reduce all these structures to a common histo-genetic origin, the author proposes to designate as extra-cellular substance, all that has hitherto been regarded as blastema, basis, and intercellular substance, or cell product. The renal and seminal canals he finds at first to consist of solid cell-masses, on the outer surface of which the peculiar secondary gland-membrane first appears as a structureless and very thin blastema layer, devoid of nuclei. Bruck also recognises the existence of a kind of connective tissue in which simple nuclei play the part of cells. Many so-called free nuclei exist, which, on further examination, are

* Edinburgh Monthly Journal of Medicine, Oct. and Nov. 1854.

† Schmidt's Jahrbücher, No. 2, p. 151. 1855.

proved to be cell-nuclei: that is, surrounded by a distinct cell membrane; but there are also to be found, for a long time, in many connective-tissue structures, not only the entire or rudimentary remains of original cell-nuclei, but also in pathological as well as in fetal connective-tissue frequently, a greater or less quantity of round, oval, oblong, and pointed nuclei, in which no cell wall can be demonstrated; and neither by acetic acid, boiling, or other means, can any trace of an investing membrane be found. These nuclei generally disappear, especially in pathological structures, after they have become elongated into small rod-like or pointed bodies.

Cell-nuclei are also to be met with, which reach an enormous length, and may properly be called fibres. That these elements are not elongated cell-membranes is evident from their appearance, and also because the fibre-forming nuclei entirely disappear under the action of potash. Further, in the nuclei of the elastic fibre cells, the pointed ends are characteristic, and they may be thus readily distinguished from the rod-like stunted nuclei of the contractile fibre cells; their sharp dark outlines, and their homogeneity stand in contradistinction to the pale, often finely-granular, or nucleated nuclei of the muscle cells. In the very open connective tissue, cells will be met with, arrested, as it were, in the process of elongation into fibres; besides the elongated nuclei, cells also are seen, the ends of which are produced into long and fine threads, the nuclei being drawn out very fine, but capable of being brought into view by acetic acid.

Bruck combats the opinion of Virchow, that the nucleus-fibres are hollow, and form a very extensive system of tubes ministering to the process of nutrition.* He has never seen any such cavities, and thinks their existence improbable, as the elastic elements do not become solidified by degrees, but are so from the commencement. According to this author, the greater part, if not the whole of the actual elementary parts, on which Virchow's theory is based, is to be referred neither to the connective nor the elastic tissues, but to another—namely, that of undeveloped capillary vessels. The author bases this opinion on the results of the investigations of the Whartonian gelatinous tissue; in very small embryos the number of the spindle-shaped cells, which constitute the vascular walls, continually decreases, until finally but one or two remain. The smaller the number of these cells, so much the thinner will be the walls of the vessel when complete, and so much the greater will be the amount of development of the individual cells, which increase much in length, and throw out thread-like processes, by which they become connected to each other. These fine connecting threads not infrequently contain single, and even many, elongated blood-corpuscles, lying in rows, and thus showing that here already a circulation has been established. According, however, to Bruck, the stellate formative-cells of Schwann have nothing to do with the first development of the vessels, but appear constantly at a later period, when the larger vascular subdivisions are completed, and their peripheral extensions are further accomplished by the continually increasing intercellular substance. In the tail of the larvæ of the frog, at the time when the heart and larger vessels are being formed, and the circulation established, there may be seen at the peripheral extremities of the vessels free branched cells, which by degrees attach themselves to the outer vascular loops, and become connected with their fine, and at first solid, prolongations.

Amongst the parts in which the vascular system never passes beyond the fetal stage of development, the capsule of the lens, and the capsulo-pupillary membrane in the first half of fetal life, exhibit beautiful examples of vessels forming broad, wide meshes, and with thin, structureless walls. The vessels of the capsule of the lens lie in the structureless membrane, and course along pretty extensively without forming branches or anastomoses; they often have many nuclei running in the direction of the long axis of the tube; frequently a vessel will be seen to run into a long, thin, solid thread, which exhibits enlargements, in which nuclei are deposited.

* British and Foreign Medico-Chirurgical Review, No. xxvii., *Annals of Micrology.*

Around the lens will sometimes be found a thick mesh-work, which, with many large branches, springs from the arteria centralis, but is not throughout permeable to blood. Often, rows of spindle-shaped cells are to be seen, which the author regards as undeveloped vessels of a larger kind; sometimes, a single spindle-shaped cell, with extensive prolongations, appears to connect two large vessels. On the capsule of the lens, no considerable intercellular substance is to be found between the vessels; but it will be seen in abundance, and in continually increasing quantity, around the arteria centralis bulbi and its branches. The above-named cells, as well as those of the intercellular substance of the Whartonian gelatinous tissue in the earlier periods, have large round or oval, never oblong or pointed, nuclei; all these cells Bruck regards as the undeveloped elements of the vascular system. Similar cells occur in the most different tissues, in the areolar connective tissue, and in the umbilical cord—in this last, in all stages of transition to complete and incomplete capillaries; but Bruck has never seen elastic fibres or contractile fibre cells in the umbilical cord, or in the Whartonian gelatinous tissue. According to these views, Bruck considers the so-called corneal corpuscles of Virchow and Strübe, for the most part, as abortive or incompletely-developed vascular elements of the cornea; and he puts the spindle-shaped appendages of the fine capillary vessels of the cornea, described by Coccia, in the same category; and likewise those fine prolongations, whether blind or anastomosing, and containing no corpuscles, lately described by Kölliker in connexion with the capillaries of the cornea. These first-described vessels are, as we have seen in inflammation of the cornea, capable of undergoing a sudden development, and then become filled with blood-corpuscles.

Besides these so-called vascular cells, which are characterized by a more or less rounded or oval nucleus, there are also to be found in the cornea, especially in the pig and calf, elements with long-pointed, sharply-contoured nuclei, which we may regard as young elastic fibres; they anastomose seldom, and run chiefly in the direction of the lamellæ of the cornea. Bruck has not found spiral fibres in the cornea; he regards this structure as one closely related to the Whartonian gelatinous tissue and the lens, but which has reached a higher degree of development, and forms the transition to the more highly-organized tissues—as, for instance, the areolar connective tissue. These undeveloped vascular cells occur in other parts of the body besides the eye structures; they are more or less constant in almost all connective-tissue formations, as those which are usually considered poor in vessels or altogether devoid of them—as the serous membranes, the ligaments; in the central organs of the nervous system—as the infundibulum; in many glands, and the marrow of bones.

The author gives the following characteristic distinctions between the spindle-shaped and the caudate cells of embryonal tissues in general, and embryonal connective tissue in particular:

The contractile fibre cells are distinguished by their permanent independent character, by their small disposition to fusion, by their remarkable bi-polar growth, and the persistence of their nuclei, which do not transgress the rod-like form, and lastly, by their well-known behaviour to acetic and nitric acids and water.

The elastic fibre cells are characterized by the almost unlimited growth of their spindle-shaped or pointed nuclei, their resistance to the action of acetic acid and potash, and their disposition to anastomose, and the formation of a network by a usually uniform bi-polar growth.

The vascular cells are recognised by their round or oval nuclei increasing by partition, by the multi-polar growth of the cell body, and their remarkable proneness to fusion.

The so-called stellate pigment cells appear to differ from the vascular cells only in the different constitution of their cell contents.

REGENERATION OF TISSUES. •

Tendons, regeneration of.—BONER* has instituted some experiments on the regeneration of tendon, for which purpose he made sections of the tendo-Achillis in the rabbit, and examined the parts at various times after the date of section. He finds that when a plastic exudation takes place, the walls of the sheath of the tendon become united, and finally degenerate into a thin solid string, the use of the tendon becoming permanently lost. On the other hand; when an effusion of blood takes place, perfect union of the divided parts is subsequently brought about. The effused blood coagulates very soon, the blood-corpuscles become disintegrated, the fibrine softens, and, after a couple of days, the whole presents a homogeneous, here and there granular, appearance. On the fourth day, the blood-corpuscles have almost completely disappeared, the whole mass being uniformly red, and filled with granules; round cells, with large indistinctly-bordered nuclei, begin to be seen, but soon lose their rounded form, and become elongated, the nuclei assuming a spindle shape. In eight to ten days, the coagulum has become almost completely white, the cells are very delicate in outline, and thin prolongations are thrown off from them, which here and there may be traced connecting one cell with another. The intercellular substance shows a clear longitudinal striation, which is more marked each day, and the whole coagulum more and more assumes the appearance of a true tendon, the tendinous tissue being completely organized about the end of the second week; it is not, however, till the fourth week, or even later, that the normal strength and consistence of tendon are assumed.

Boner concludes from his investigations, that the structure of tendon, like that of the cornea, is made up of flat stellate cells, or a fibrous intercellular substance.

Regeneration of Nerves.—The following observations will prove of value for the purpose of comparison with those of Waller and Budge, which we formerly recorded. SCHIFF* considers that the regeneration of nervous tissue takes place by the formation of new fibres in the old sheaths, even before the previous ones are completely disorganized. At the seat of section, the parts become red, and somewhat swollen; and between the bundles of fibres, small, rounded, or angular nuclei, with nucleoli, show themselves; between them will be seen a quite structureless mass, resembling connective tissue, in which soon appear nuclei in rows, and at first round, but afterwards oblong, and placed opposite, but in alternate order. On the appearance of these nuclei, the mass becomes separable into laminae, in which the continuation of the primitive fibres of both ends of the nerve are visible. While at first the whole mass comports itself to potash and acetic acid, like areolar tissue, the cylinder fibres now, on the addition of potash, distinguish themselves from other fibrous tissues, assuming a pale yellow colour, and clear contour. These stages of the development proceed from both ends to the centre uniformly, but the following changes take place more rapidly in the upper. The substance lying between the rows of nuclei, acquires a finely striated appearance, and on the side of every finely striated portion may be seen a dark line, which is the expression of a membrane in which the nuclei are placed. The cylinders assume by degrees a somewhat marked, pale greyish-yellow colour, so that the new nerve has now the appearance of the primitive fibres of the olfactory, and, like this, shows here and there indications of a double border, but it is not so dark; the outer line of this double border is stronger than the longitudinal striation in the substance within. Potash brings out the cylinders very clearly, but does not attack them. Acetic acid shows the nuclei distinctly. Somewhat later, the dark investments of their contents (axis-cylinders) seen in the single nerve fibres are, as it were, removed, and between them lie broad quadrangular, or somewhat rounded, laminae, which are found to be fat; when these laminae exist, the nuclei of the sheath are no longer visible; their number increases, and they at last become

* Virchow's Archiv. Band vii. Heft 1, p. 162.

† Schmidt's Jahrbücher, No. 2, p. 284. 1854.

united to the sheath, which is at first very thin, and but slowly increases in thickness, and hence the newly-formed nerves for a long time exhibit a small diameter. This author does not agree with Kölliker in the opinion, that the formation of a nervous cicatrix is quite parallel to that of embryonal nerve tissue. As to the period at which a regenerated nerve is capable of reassuming its functions, Schiff has observed that, in the infra-orbital and lingual nerves, the communication of sensations was re-established before the regeneration of the marrow in the new part was completed, and at a time when only single fat particles showed themselves in parts remote from each other. The sensitive, and probably also the motor, communication, would seem to require, not the marrow, but the axis-cylinder. The period occupied in the regeneration is from eleven to seventeen days; it is shorter in wounds by section than in those by laceration. The vascular nerves heal more easily and quickly than the sensitive, and these, again, than the motor nerves.

These observations are very well borne out by the investigations of Bruck,* on the sciatic nerve of a cat, which had been cut across in the middle of the thigh, and examined some months after, when the powers of the extremity had been completely restored. The regeneration appeared to be as complete as possible, union having taken place fibre for fibre, there being no blind or ununited extremities; and in each fibre the cicatrix was still visible where the junction of the divided parts had been effected. Nowhere was it observed that two or more fibres were united together, nowhere was there any intermediate substance, exudation, or callus; each end of a central fibre had again found a peripheral fibre, with which it had united so as to form a continuous and isolated line. Above and below the cicatrix the fibres in all these parts, and in all their relations, were perfectly normal. The seat of the cicatrix was marked by a more or less deep circular constriction of the nerve tubes, which on both sides of it were somewhat swollen out, and flask-like. The marrow was in all the fibres, as well above as below the cicatrix for a short distance, somewhat granular, finely striated, and presented a double contour; but at the point of section, and in the dilated parts, it was completely clear and transparent. In this clear space, without the application of reagents, and without further preparation, the axis-cylinder could in many instances be seen, its diameter sometimes unchanged, or, as it might be, a little enlarged or diminished. In a few instances it stopped short on one side or the other, and was no longer visible. The regeneration was most complete in the outer sheath and the axis-cylinder, while the nerve-marrow was sometimes not complete, or replaced by another transparent substance.

HISTOLOGY OF ANIMAL FLUIDS.

Blood-corpuscles, enumeration of.—Vierordt† continues his researches on this subject, notwithstanding the strictures of P. Dubois-Reymond and Ludwig. He has slightly modified his processes. He dilutes the blood still more than formerly, namely, 679·9 times; he uses for this purpose a solution containing $2\frac{1}{4}$ grammes and 0·16—0·17 gramme of salt, in 100 cubic centimetres of water. The capillary tube employed has a diameter of half a millimetre, and its size is measured by being filled with quicksilver, instead of by micrometry.

Amongst various results of his investigations, the following is very remarkable. He finds that the quantity of the blood-corpuscles and the colour of the blood in one and the same animal, stand in no simple relation, nor are they reducible to any law; pale blood was generally found richer in coloured corpuscles than would have been expected from its colour. These results were so contradictory, that Vierordt gave up the attempt to determine the relation of the quantity of corpuscles to the colour of the blood. In three animals bled to death, death ensued in two as soon as the number of the corpuscles sunk to 52½ of the normal number.

Decrease of Blood-corpuscles during Hybernation.—In a marmot examined the 28th of November, the mean number of blood-corpuscles in the cubic millimetre

* Schmidt's Jahrbücher, No. 10, p. 789.

† Ibid., No. 1, p. 4. 1855.

was found to be 5,828,000; on the 5th January this number had decreased to 5,106,000; on the 4th of February, to 2,356,000. Hæmorrhage appears to have taken place during the last operation. The animal's weight also was observed to diminish at each period.

Proportions of coloured and colourless Corpuscles in Splenic Vein.—The mean of four enumerations by Vierordt on the body of a criminal, gave 4·9 colourless to 1 coloured.

*Lymph, Micrological Characters of.**—A rare and very remarkable case of lesion of the lymphatics has given to M. Gubler an opportunity of analysing this fluid, so seldom in the human subject recorded to have been submitted to chemical or microscopical examination. We, therefore, produce here the most important results of the observation.

A female, in a good state of general health, presented, at the anterior and superior part of the left thigh, about two centimetres below the fold of the groin, several small phlyctenulæ, or translucent vesicles, of the appearance and size of a grain of sago boiled. They were covered only by the epidermis, and appeared manifestly to result from a varicose dilatation of the sub-epidermic lymphatics. They were disposed in two divergent lines, enclosing a very acute angle, the apex of which corresponded very nearly with the opening of the internal saphena; the sides diverted outwards were lost before they reached the inguinal region; the superior followed exactly the fold of the groin; the inferior ran a little more transversely. There were four very apparent and prominent vesicles a little lower down, and lying on the border of the sartorius. The largest amongst them, when torn with the point of a needle, gave exit immediately to an opaline fluid, which ran in a little stream down the thigh, falling at the rate of about fifty drops per minute, and this continued until arrested by compression. The smaller vesicles gave exit to but a very small quantity. This liquid, collected in a vessel, coagulated, in from one quarter to half an hour, in the same manner as blood—that is to say, it formed a clot swimming in a fluid; but the serum, so to speak, retained the same colour as the clot, was undistinguishable by the sight, and could only be recognised on shaking the mass.

Submitted to chemical and microscopical analysis, this fluid was found to present the characters assigned to lymph. Other abnormal conditions of the lymphatics existed in this limb which it is not necessary to specify here. It may be mentioned, however, that on one occasion lymph escaped from a rupture of one of the vesicles, and continued to flow for forty-eight hours; and it has been calculated that about 2830 grammes, or nearly six pounds, escaped in the twenty-four hours.

Physical properties.—At the moment of exit from the vesicle, the liquid was white, opaque even when seen in drops, having much the appearance of milk deprived of its cream, with a slightly dull yellow tint. It had a strong alkaline reaction, a feeble saline taste, and a scarcely sensible animal odour. As before observed, it separated into a serum and clot, the former still retaining the opaque white colour, and scarcely distinguishable from the latter.

Microscopic examination, with powers from 300 to 500 diameters.—A considerable quantity of yellowish corpuscles occupied the field, similar to those of blood recently withdrawn from the circulation, but of very unequal size. Some had the usual dimensions of blood-corpuscles, but the greater part were sensibly smaller, reaching a diameter of only $\frac{1}{500}$ th of a millimetre. Lastly, there existed a certain quantity of coloured corpuscles, like the preceding, but much smaller, being only about half the dimensions in diameter of the larger ones—that is, about $\frac{1}{1000}$ th of a millimetre. These little globules were seen to be rounded on all sides as they rolled over the field; they were constantly spheroidal, and exhibited no flattening, excavation, or anything which indicated a nucleus; their surface was smooth, their outlines regular, and their yellow colour appeared as intense as that of the best formed blood-corpuscles, if not even more so.

Besides these coloured corpuscles, which, in the opinion of Gubler and Qué-

venne, are to be regarded as only modifications of those of the blood, there were others less numerous, and pale or colourless, and of very various dimensions. The smallest, having the same dimensions as those last described, were white or colourless at a certain focal distance; but on approximating the object-glass a little towards them, they appeared to present a very light greenish tint. They were spheroidal, covered with small, but very apparent, granulations, which, however, did not render their outline irregular. The largest of these white globules exceeded in size the largest blood-corpuscles. Their form was regularly spherical, their outlines smooth and uniform, and their walls finely punctuated. There was no visible nucleus, and no appreciable greenish tint. These larger white globules, reaching to $\frac{1}{100}$ th of a millimetre, were very rare; but the smaller white ones were present in abundance: intermediate ones were constantly seen. Lastly, there were suspended in the liquid, infinite multitudes of molecular granulations, scarcely visible from their extreme tenuity, reaching to only about $\frac{1}{200}$ th of a millimetre in size.

The above elements may be regarded as those proper to an average specimen of this fluid, but on some occasions others were observed. Thus, in one examination there was found a considerable number of discoid blood-corpuscles, and bodies analogous to the white globules of the blood; in another specimen the small white globules, and the small spheroidal bodies of a hæmatoid nature above noticed, were less numerous. With these exceptions, the same elements were constantly to be found, and with the same characters, the relative proportions of them alone varying.

Certain changes were produced in these little bodies when allowed to rest, all, however, more or less attributable to histolytic influence, and, as we conceive, in no way characteristic; the discoid bodies became globular, granular, and mulberry-like, and were at the same time diminished in diameter. The small spherical blood-globules manifestly multiplied as the lenticular bodies disappeared; a part of the molecular granules grouped themselves into pellicular masses, more or less extensive, in which were to be seen some very brilliant globules (oil particles).

Weak acetic acid dissolved almost all the red globules, which first became enlarged; a few of the colourless corpuscles resisted its action for some time, but finally disappeared. The white globules were greatly altered by this reagent, but did not become dissolved; the largest became much swollen, their cellular wall becoming pale and thin, and losing its punctuated appearance. The granulations assembled in the cavity of each cell in a single rounded mass, resembling in form and aspect an ordinary ex-centric nucleus. In the small white globules the granulations of some became more apparent on the addition of the acid; others exhibited a very pale zone, which seemed to be a rudimentary cell-wall, closely embracing a granular nucleus. By the action of ammonia, also, the red globules instantly disappeared; the white globules dissolved in this reagent, but more slowly. The molecular granules were dissolved by ether, oily drops resulting on its evaporation. Iodine coagulated the albuminoid matter, and coloured it yellow, as well as the globules. Water swelled out the discoid bodies, and rendered them vesicular, depriving them at the same time of their colouring matter.

The clot was composed of a mass, which appeared amorphous when thick, but which was manifestly fibrous and striated when examined in thin particles, and showed delicate fibrille under the microscope. The following is a *résumé* of the observations on the microscopic elements of this lymph.

It contained, in suspension in a serous liquid:—(1) hæmatoid corpuscles, always of a diameter inferior to those of blood, some lenticular-like blood-corpuscles, properly so-called, others very small, spheroidal, and smooth; (2) pale, scarcely-coloured globules, being those more usually designated as lymph-corpuscles, some exceeding in size the red corpuscles of blood, others much smaller; (3) granular fatty molecules.

The authors regard the first-named elements merely as modifications of the blood-corpuscles, presenting a similar aspect and similar chemical reactions; the second resemble the white corpuscles of the blood, but differ from them in certain

regards; these are the veritable corpuscles of lymph of authors. The difference between the white globules of the blood, and the large white globules of lymph, seems, according to MM. Quévenne and Gubler, to be manifested chiefly in the different comportment of their nuclei to acetic acid; but they do not attach much importance to it.

CARTILAGE AND BONE.

Cartilage and Bone, Structures of.—Bruck,* after pursuing his views on the connective-tissue elements, describes those of bone and cartilage, especially in their relation to each other, and advances some views different in many most important respects from those generally entertained. He considers that all the primordial parts of the vertebral skeleton, as far as their ossification is concerned, again disappear, and become fused into a secondary blastema, out of which the so-called bone and cartilage marrow tissue are produced. All that is comprised under the term bone in the adult, is, with few exceptions, an entirely independent, generally later formation, which from the commencement is produced as bone, and is either entirely independent of the pre-existing cartilaginous parts, or is developed as an "apposition" to them; consequently, to refer the bone-structures with their medullary canals, or the bone-corpuscles, to the elements of cartilage, is, in his opinion, untenable; and the cartilaginous pre-formed bone, when it remains as such, contains no true bone-corpuscles, but only ray-like, round, or oval ossified cartilage cavities. The author therefore distinguishes the proper bone-tissue from the ossified cartilage, even when the so-called secondary formation of bone is produced by the deposit of an organic cartilaginous base. The secondary bone is neither ossified connective-tissue nor ossified fibrous cartilage, but an independent form of tissue. Ossification and fibrillation are in no way connected; but, on the contrary, the more cartilaginous, as well as other, tissues undergo ossification, the more fibrous do they become. The bone-corpuscles, then, are not ossified cartilage-cells, but independent formations.

NERVES.

The observations of Gegenbaur, Kolliker, Leydig, H. Müller, and Virchow, on the body of a beheaded criminal, though chiefly of value in a physiological aspect, throw light on some hitherto obscure points in normal histology.

Retina.—In some investigations on the retina, it was found, that in the neighbourhood of the yellow spot, the cones are smaller, but of greater length, and closely pressed together, while, external to it, the rods become interposed. The cones in this situation were scarcely pyriform, had a pretty uniform thickness of 0.002", had no point, and were from 0.012"—0.014" in length. At the periphery of the retina, these little bodies were thicker, assumed rapidly a more pyriform or oval shape; so that, with a length of 0.008", their greatest breadth was 0.003"—0.004". Their somewhat conical points, which, in the peripheral parts, were marked off by a transverse line, had a length of about 0.006"; the isolated rods were about 0.0008"—0.0009" in breadth, and 0.012"—0.014" long. It was also observed, by careful focalizing, that the points of the cones lay somewhat deeper than the extremities of the rods. Vertical sections through the yellow spot showed a remarkable thinness in this situation; the yellow diffused colour had its seat in the middle layers, but little, if at all, in the inner cell and outer rod-layers.

Nerves of the Papilla of the Cutis.—Kolliker is of opinion, from the result of his examination of the parts in a recent state, in the decapitated case already noticed, that the transverse strim of the corpuscula tactus of Meissner are nuclei, which lie in spindle-shaped connective-tissue corpuscles, and that they are by no means to be regarded as nerves, as Meissner and others have supposed. The nerves often lie in spiral coils around the corpuscula on the outside, but at other times run straight beside them; their terminations could not be clearly defined.

Retina, Histology of Yellow Spot.—Bergmann,† of Rostock, had an opportunity of investigating the structures of the eye in the case of a beheaded criminal, six hours after death. His views differ somewhat from those of Kolliker.

* Loc. cit.

† Henle und Pfeuffer's Zeitschrift, Band v. p. 245.

Half of one retina was examined fresh; the section had been made through the middle of the yellow spot, behind which only small cones were found, some with numerous rows of rods between them; but this part of the investigation appears to have been very incomplete, owing to the structures breaking up into fragments, the localities of which could not be determined.

The second eye was prepared and hardened before section, and consequently the retina exhibited no folds, but there were some differences of level, owing to the difference of thickness in different parts, while, probably, a great part resulted from the action of the chromic acid. The small angular fovea centralis lay very sharply defined in the middle of a somewhat pyriform field, the point of which was turned towards the colliculus of the optic nerve. This space was bounded on its upper and under sides by delicate borders, which did not reach completely to the point; but there intervened between them here a middle elevation, the plica centralis of former observers. The borders inclined towards each other, but did not touch. Bergmann proposes to call this little field, the Area centralis retinae. In this area the nervous layer becomes suddenly very thin. Some sections from the optic nerve to the fovea showed well the particular disposition of the nervous matter in this situation; the layer of ganglionic bodies was found not to be continued over the base of this depression, while both the granular layers, with the intergranular layer, though very thin, were continuous throughout. Bergmann uses this as an argument against the opinion, that the ganglionic bodies constitute the perceptive part of the retina. Kölliker, Hannover, and others, consider the fovea as a physiologically imperfect part. Bergmann describes a peculiar arrangement of the fibres in the neighbourhood of the fovea, by which those coming from the outer granular layer take an oblique course, and finally pass into the inner layer. This observer claims for this portion of the retina a higher physiological importance than is accorded to it by others.

*Nervous Ganglia of Lymphatic Glands.**—Schaffner asserts that further researches have confirmed him in his opinion of the existence of microscopic ganglia in the lymphatic glands. He has, by degrees, brought the whole of some of the minute axillary glands of the mouse under the field of the microscope; a section being made longitudinally, and the structures treated with acetic acid, nerve fibres and ganglia were found, but very sparingly.

Périnèvre.†—Under this name, M. Robin describes a structure investing the primitive bundles of the nerves, forming an uninterrupted sheath, which extends from the point of exit of the nerves from the dura mater or the ganglia, to the peripheral terminations of the nerve-tubes; it is absent from the branches of the sympathetic which present a grey colour and soft consistence, but exists in those which are white. Each tube is composed of a wall, some thousands of a millimetre thick, homogeneous in substance, and having neither fibres nor fissures; it is, however, slightly granular, and provided with longitudinal nuclei, much further separated from each other, and much less numerous in proportion as the tube is large. It is rendered hard, and assumes the appearance of parchment on the addition of nitric acid. Each nerve filament, whether visible to the naked eye or not, is enveloped by this périnèvre; it is to be distinguished from the neurilemma, which forms a sufficiently thick coat on the large nerves, and supports the nutrient vessels, but the périnèvre exhibits no capillaries.

VARIOUS STRUCTURES.

Structure of the so-called Worm of the Dog's Tongue.‡—It may be interesting to mention here the results of a microscopic examination of this little body, made by VIRCHOW. § He has found it present in all dogs, as well old as young; its phy-

* Henle und Pfeuffer's Zeitschrift, Band v. p. 255.

Gazette Médicale de Paris, No. 38. p. 589. 1854.

‡ Virchow's Archiv, Band vii. Heft 1, p. 170.

biological use he considers, with Morgagni, to be that of furnishing a support to the very long tongue of these animals. Two structures may be discerned in it by the naked eye: one, red, fleshy, and separated into irregular divisions; the other, white, hard, and apparently cartilaginous. In the former, Virchow has found striated primitive muscular fibres, which pass from side to side in rather sharp curves; abundant nerve filaments were seen; there were also some longitudinal muscular fibres. In the dense white part, hitherto generally considered cartilaginous, he found no cartilage elements, but a thick fatty tissue, enclosed in a dense fibrous covering, from which the muscular tissue springs.* Virchow, therefore, regards this little body as neither sinew nor cartilage, but as a quite peculiar, half-muscular, half-fatty and fibrous structure.

DONDERS* finds that the bile contains no hepatic cells, but only cylindrical epithelium, with nuclei. The liver cells are, in the higher animals, subject to molecular changes. In the secretions of the pancreas, and in the saliva, some dissolved and half-disintegrated gland cells are constantly found. He considers the mucus of the intestinal wall to be produced by the rupture of distended cells on the free surface; some cells burst and discharge their contents without losing their nuclei.

In the very open tissue of the papilla the fat, after absorption, becomes heaped up into large drops (it is usually infiltrated uniformly between the bloodvessels), and these form, by flowing together after death, a kind of compound cells.

The papilla possesses very numerous capillary vessels, which lie close under its delicate investing membrane; lymphatic vessels are, according to Donders, but rarely seen in the papilla, and then but as a central vessel near the branch of the bloodvessel: he has not observed a network commencement of the lymphatics in the papilla. The pancreatic juice has not, in his opinion, any particular import in the absorption of the fat. He confirms the presence of fibre cells and a contractile power in the papillae, but denies the existence of open mouths; though, as in his former experiments with *Mensouides*, he has often observed the entrance of solid molecules.

Structure of the Lymphatic Glands.—DONDERS† confirms, generally, the opinions of Reichert and Kolliker on the structure of the lymphatic glands. He finds them to possess a strong, but very thin capsule, in which he has not seen fibre cells, though he in no way throws doubt on the statements of Heyfelder. From the capsule, bands pass into the gland tissue, which divide it into lobules. The external lobules are bordered all round by the capsule, the internal not entirely so. The contents of the lobules consist chiefly, but not entirely, of cells lying in a little fluid; between the cells there is a prolongation of the capsule in the form of a very fine stroma, consisting of a network of fibres, with very large meshes. In the outer lobules, capillary vessels are found. After ligature of the ductus thoracicus, the lymphatics of the gland become filled. The lymph and the chyle do not, in the opinion of Donders, entirely pass through the glands, but find a circuitous channel. In coagulated, as well as in injected glands, he has found the contents of the vessels to escape, and to become infiltrated into the parenchyma. The lymph passing out of the glands contains less water, and more fibrin and morphological elements—namely, lymph-corpuscles, which afterwards become blood-corpuscles.

Peyer's Glands.—DONDERS‡ agrees with Brücke in considering these glands as entirely analogous to the outer lobes of the lymphatic glands. They possess a stroma, and capillary vessels, take part in the fatty absorption, and stand in connexion with the lymphatic glands.

Corpora Amylacea.§—We have hitherto found these bodies noticed as occurring under conditions little, if at all, departing from the normal; GÜNSBURG has, how-

* Schmidt's Jahrbücher, No. 9, p. 286. 1854.

† Ibid.

‡ Ibid.

§ Ibid., No. 10, p. 16. 1854.

ever, found them to constitute a large element in certain tumours in the brain, near the vessels of the neck, and behind the stomach.

Cilia: Nasal Membrane.—GEGENBAUR* was unable to detect any ciliated epithelium on the upper eyelid, but found, in this situation, the ordinary pavement epithelium, which corresponds with Henle's later observations.

According to the observations of Gegenbaur, Leydig, and Müller, a ciliated epithelium is found over the whole nasal mucous surface, even in the olfactory region on the cribriform plate, which, according to Todd and Bowman, possesses, in lower animals, a peculiar, unciliated cell-layer.

It was endeavoured to determine the direction of the ciliary wave by finely pulverized carbon, but no results were obtained to warrant a belief that the stream took place in any determinate direction. The epithelial cells in the olfactory region appeared to be more delicate than in the under parts, and here and there had two, and sometimes three, nuclei.

Cilia: Auditory Membrane.—KÖLLIKER, in the same case (that of the beheaded criminal), examined into the distribution of ciliary motion in the auditory apparatus, and showed that it was absent from the membrana tympani; neither could it be demonstrated to exist in the ossicula: it was present, however, on the promontory, on the outer wall near the membrana; and also on the floor and roof of the cavity.

Cilia: Brain.—LEYDIG examined this organ with a view to determine the question, whether cilia exist on the surfaces of the ventricles, and, if so, in what situations. This observer had found that in fishes, amphibia, and birds—and, according to Valentin, in mammalia—the epithelial cells of the choroid plexus were ciliated; but, though the cells of the choroid plexus, in the case under examination, were well seen, and presented a clear border, there were no cilia to be seen in this situation, nor in the cells of the ependyma of the lateral and third ventricles, though fine vertical sections were made, which brought clearly into view the sharp border of the cell-layer. In the fourth ventricle, however, ciliary motion was distinctly observed; the cells were of a round, flat form, the cilia were tolerably long, but very fine, and, when moistened with blood-serum, moved with great activity—a fact which was verified by Müller, Kölliker, and Virchow. When the cilia became inactive, their motion could be once again, for a short time, vigorously re-established by the application of a solution of potash. This ciliary motion, it is to be remarked, was chiefly confined to the neighbourhood of the striæ acousticae (calamus scriptorius).

Kidneys.—LEYDIG could find no trace of ciliary motion in these organs.

• *Conarium.*—M. FAIVRE† recognises three elements entering into the structure of the conarium in man and animals: a fibro-vascular capsule, a soft parenchymatous matter, and an inorganic substance. The parenchymatous matter, hitherto generally regarded as formed by the grey substance, is found to be of a completely different nature. Under a power of 500 diameters, it is seen to be invariably composed of rounded or oval bodies, with clear outlined borders, and variable size; they are insoluble in water, nitric acid, and alcohol. The author, therefore, considers the pineal gland in man, as well as in animals, to be formed by a quite peculiar histological element, distinct from that which enters into the composition of the nervous tissue.

Choroid Plexuses (Structure of).‡—FAIVRE distinguishes three elements in the choroid plexus in the human subject, which he describes as the choroid villositæ, the choroid mass, and the choroid vesicles. The last exist only in the human subject, being found in the lower part of the lateral ventricles, under the form of

* Observations on the Body of a Beheaded Criminal: Verhand. der Phys. Med. Gesellschaft zu Würzburg, Band v. Heft 1, p. 14. 1851.

† Gazette Médicale de Paris, No. 36, p. 555. 1854.

‡ Ibid.

a cluster, which, when closely examined, is seen to be formed by a fold of cellular lamina, adherent in certain places to a subjacent vascular basis, the free portions thus assuming the shape of vesicles; each of these apparent vesicles contains a soft white mass of cellular tissue, and a large quantity of small inorganic granules, partly composed of carbonate of lime. In man, as well as in the higher animals, numerous concretions are formed in the choroid plexus, sometimes produced in the cells of the epithelium (?), sometimes in the vesicles, or attached to the walls of the vessels, or free in the spaces comprised between the coils of the vessels and the epithelial layer. These concretions consist of carbonate of lime, phosphate of lime, cholesterine, silica, phosphate of magnesia, carbonate of potash, and ammonio-magnesian phosphate.

M. Favre thinks that the choroid plexuses, the surfaces of which are so much increased by the villosities, have a close relation to the production of the cerebro-rachidian fluid.

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

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1. On *Herpes Tonsurans* (Cazenave). By Professor HEBRA. (Zeitschrift der K.K. Gesellschaft der Aertze zu Wien, December, 1854.)

PROFESSOR HEBRA, in this paper, gives first an historical account of the disease which, under the various names of porrigo scutulata, ringworm, teigne tondante, tinea tonsdens, trichophyton tonsuraus, rhizophyto-alopecia, has been the subject of so much discussion.

Willan described the disease correctly, except that he derived it from little achorous pustules, an unusual commencement. Plumbe gives a very good account of it. Alibert, apparently, was little acquainted with it; and Raycr, who gave it the name of teigne annulaire, erroneously considered it, with Willan, as a pustular affection. Later authors, as Green, Gilbert, Riecke, Wilson, Fuchs, have described the disease either as favus, or as a species of herpes. In 1840, Cazenave described the disease under the term herpes tonsurans, although he knew it had been called porrigo scutulata by Willan, and teigne tondante by his countryman Mahon. In 1845, Malmsten discovered a fungus in the roots of the hair, and Gruby soon afterwards confirmed the observation. Very lately, Robin and Bazin have also carefully described the cryptogamic plant, though the plate given by Robin does not (according to Hebra) represent the fungus of herpes tonsurans, but that of *plica polonica*.

Hebra then describes the *symptoms* as follows:

1. On hairless parts there are two forms.

(a) Vesicles with clear, or sometimes yellow, contents, grouped or isolated, or normally coloured on reddened patches of skin. Except in paucity of number, these vesicles are not different from those of herpes preputialis, labialis, &c. In a few hours, however, they dry and form a thin, yellow-brown scurf, which is sometimes surrounded by a ring of vesicles, which rapidly run through the same course. The vesicular character is so decided, that Hebra thinks Cazenave quite justified in the name he has adopted.

(b) More frequently than the vesicular form of the herpes tonsurans is the second or macular form; small deep-red spots, elevated in the very slightest degree above the level of the skin, form and become covered with thin white scales. In a few days, the increase of the spots at their borders has augmented the size of the patch to that of a sixpence, the outer border being more defined and redder than the centre. Afterwards

the centre loses its clear-red colour, and becomes bluish-red, or yellow, or even natural. This appearance characterizes the ringworm of the English. Frequently many maculae begin at once; they pass and fuse into each other, and give rise to various shapes.

2. On hairy parts the herpes tonsurans is chiefly distinguished from the disease on hairless parts by the dry, ragged remains of hairs of unequal length, as if the hairs had been cut off unequally; the hairs also often drop off, and the yellow-white, or yellowish-brown, paper-thin, dry, bran-like scales of cuticle are disclosed.

The diagnosis of the disease on the hairy parts is especially given by the condition of the hair; on hairless parts, the number, arrangement, and figure of the vesicles, or the colour, form, size of the spots, and the grouping of the patches, with the microscopic characters, suffice for the determination.

The microscopic examination of the cuticle, or of the roots of the hairs, or in the hairs, discloses, with a little care, the fungus, which is the smallest at present known.

The cause of this disease is unhesitatingly referred by Hebra to the growth of a fungus finding a fit soil. We have, therefore, only to learn how and in what manner the soil is prepared for the fungus, and how the fungus gets there. With respect to the first point, Hebra points out that a macerated cuticle represents the fit receptacle; and if in any way the cuticle becomes moistened, the fungus will grow, if it can arrive at the part. He calls particular attention to the fact that when fomentations are continually applied to limbs, an eruption of little vesicles or maculae often appears; in many cases these resemble closely both favus and herpes tonsurans, and on microscopic examination, the fungus can be detected. In fact, this eruption is owing to the fungus, which finds a fit receptacle in the macerated cuticle. As to the mode in which the fungus arrives at this cuticle, there can be no doubt that in many cases it is brought on the fomenting cloths. In other cases, its presence cannot be so easily accounted for.

Sometimes favus and herpes tonsurans occurs at different parts on the same individual, and Hebra evidently inclines to the opinion, though he will not absolutely decide the point, that the two diseases are owing to the same fungus, at different periods of its growth.

The treatment of ringworm is said to be very successful. No internal remedies are given, but the cuticle and the hair are both removed, by applying a mixture of caustic potash, and lard. This ointment is rubbed for ten minutes, night and morning, on the head, during four to six days; a flannel cap is worn during the days the rubbing is carried on, and for several days after, until the cuticle is detached, and the normal-coloured skin is seen below. The head is washed with warm water, and the cure is complete. The entire time occupied is about twelve days. If the disease be on the scalp, the hairs must be pulled out.

2. *A Case of Cancer of the Pulmonary Artery.* By Dr. A. WERNHER. (Henle's Zeitschrift für Rat. Med., Band v. p. 109.)

A MAN, 22 years of age, came under observation with a large and rapidly-growing encephaloid tumour of the knee and tibia, for which amputation was recommended, but had not been performed, when, on January 27th, five days after the patient was first seen, he was attacked with sudden pain in the cardiac region, just to the left of the sternum, with dyspnoea and rapid respiration. Auscultation and percussion disclosed no signs. On the following day, the pain extended to the right side; there was no cough or expectoration, no cardiac palpitation, increase of precordial fulness, or murmur. The pulse was 140. The following night there were many severe attacks of dyspnoea, in which the pulse was scarcely to be felt. On the third day after the attack, some pure coagulated blood was coughed up, and on the following day, a smaller quantity of blood was expectorated. Two or three

days subsequently, the dyspnoea diminished, and the patient passed nearly into the same state as before the attack.

All this time the tumour of the knee had been growing, and on the 7th February, amputation was performed. It was found to be an exquisite specimen of encephaloid;—but we pass over its microscopic characters. On the following day there was much fever, and on the 10th February, a return of hæmoptysis. The febrile symptoms continued, and there was increasing weakness till the 19th, when there was shivering, and dull percussion-note of the bases of the lungs. On the 20th and the following days, very frequent cough, serous, bloody, offensive sputa, mucous râle all over the lungs, shivering, heat, miliaria. On the 24th, death occurred, with the symptoms of asphyxia and profound collapse.

On post-mortem examination, the iliac and crural veins and their branches were normal, not thickened, and without coagula; the vena cava was also perfectly healthy. The heart was healthy. The pulmonary arteries, on the other hand, contained coagula of coherent cancer-masses, forming fibres and strings of dull-white colour, like boiled rice, which strings were composed of many thinner ones, closely pressed together. These masses filled almost all the branches of the right pulmonary artery; the left pulmonary artery contained also many, but was freer. The walls of the vessels were normal in most cases; in some these were incorporated with the contained cancer masses. The smaller branches were dilated, from the pressure of the masses. The capillaries and the pulmonary veins were perfectly free. Under the microscope, the masses were found to be made up, almost throughout, of cells, exactly like primary cancer-cells, large, oval, with one or two nuclei. In the blood of the right heart, and of the vena cava ascendens, precisely similar cells were found. Nothing similar was found in any other blood.

Besides this, there were large gangrenous abscesses in the lungs; and it was noticed that the arteries leading to them were particularly obliterated by the cancer-masses, and that the vessels in their walls were in the same state.

There was no trace of either young or old tubercle.

This singular case, then, consisted in primary cancer of an extremity and secondary cancer in the branches of the pulmonary artery; and all the facts seem to show that the cancer-cells passed as such from the primary growth through the medium of the venous blood, to the right side of the heart, and then into the pulmonary arteries. A general infection of the blood is negatived by the fact that nowhere, except in the direct track of the venous blood coming from the tumour, were cancerous masses found. In fact, an infection of the blood, a general cancerous disease, does not appear to have gone before the primary local manifestation in the tibia. Dr. Wernher, we may remark, pauses at this place to argue against the hypothesis of a general cancerous disease, or cachexia, preceding in any case local disease; and urges that the cancer is in fact first a local one, like syphilis, and that the general cachexia is entirely secondary.

In the case now related, it would appear that after the removal of the tumour of the leg, the secondary cancer of the pulmonary artery grew very rapidly.

Dr. Wernher then compares the symptoms of his case with those of cancer of the lung given by Walshe, and finds a remarkable similarity, except that there was superadded gangrene, from the blocking up of the arteries.

3. *Communication of the Left Ventricle with the Right Auricle.* •By Dr. BUHL. (Henle's Zeitschrift für Rat. Med., Band v. Heft 1.)

DR. BUHL relates the case of a girl, aged 19, who had suffered almost from birth from palpitation, dyspnoea, and cyanosis. When visited a short time before her death, the following physical signs were noticed: considerable bulging of the thoracic wall, on a level with the second rib; dull percussion-note over the whole sternal region, from the second rib downwards, and on either side, to half an inch outside the nipples; pulsation over the whole front, more on the right than on the left

side; marked systolic thrill, more marked also on the right than the left side; marked systolic murmur on the right side, with its maximum close to the sternum, on a level with the fifth cartilage; moderate fullness, without undulation of the cervical veins; slight visible pulsation of the carotids, but of no other artery. On section, the right auricle was found of enormous size; the left auricle was extremely small; the left ventricle was somewhat hypertrophied; at the upper part of the septum the muscular substance was transformed into a thick fibrous tissue, in the middle of which was an oval opening (one-and-a-quarter centimetres long, half a centimetre broad), surrounded with a firm cartilaginous ring, and leading into the right auricle, immediately above the insertion of the tricuspid; the aortic valves were thin; the right ventricle was dilated; the tricuspid valve greatly thickened, particularly near the point where the opening existed; at this point, also, it was rigid, shortened, and therefore incompetent; the calibre of the pulmonary artery was increased, that of the aorta much narrowed; the jugular veins were not remarkably enlarged. This condition sufficiently explains the physical signs.

During the systole arterial blood must have been mixed in large quantity with the venous blood in the right auricle; during diastole a small quantity only of venous blood would pass from the auricle into the ventricle, as the entrance into the right ventricle was so easy. Professor Bull points out, that in this way the arterial was less contaminated with the venous blood than the venous with arterial blood, and he conjectures that the transmission of blood already surcharged with oxygen through the lungs may have produced the dyspnoea; as if it had been surcharged with carbonic acid. If the dyspnoea was not owing to this cause, it must have been to the pressure of the heart on the lungs.

The cause of the opening is referred, not to congenital defect, but to inflammation and ulcer-formation in the ventricular wall, occurring during fetal life or soon after birth; and the observations of Dittich are referred to, who had remarked that an abscess at this point of the heart might cause a communication between the left ventricle and the right auricle.

4. *Notes on Twenty-two Cases of Disease of the Heart in Hindoos.* By JAMES HINDER, Esq., Sub-Assistant-Surgeon. (*Indian Annals*, No. III. p. 69.)

THE following extract gives the principal facts of this interesting paper; in which, as in the essay of Dr. Morehead, recently analysed by us, we find abundant evidence that our experience of rheumatic affections in this country does not run counter to that of our Indian brethren.

"Of these cases, recent inflammation of the endocardium, or of the muscular structure, was not observed in a single instance.

"There were three cases of acute pericarditis; all terminated fatally, and were examined after death.

"On examination, extensive disease of the pericardium was observed; in front it was loosely attached to the heart by recent adhesions of soft lymph,—laterally the attachments were closer. On laying the pericardium open behind, about six ounces of pure pus gushed out; the walls of the abscess were formed by the visceral and parietal portions of the membrane, which were here greatly thickened.

"The walls of the ventricles appeared to be thinner than natural, an old deposit was noticed near the free edge of the mitral valves. No other abnormal appearances were observed in the organ.

"The liver was greatly congested.

"Rheumatism was associated with one of these cases. One patient was twenty years of age—not the case of rheumatic pericarditis,—the other two men, thirty-five years each.

"In all the other cases, with one exception, there was evidence of disease of one or more of the valves of the heart.

"Five of the thirteen cases in which chronic disease existed, or more than one-

fourth, could be traced to rheumatism as a pathological cause, the patients having, as usual, been attacked long before they came under treatment, at periods varying from five months to ten years. In most of the other cases the history was very obscure, and could not be depended on with any degree of confidence.

"Age does not appear to have had much effect as a predisposing cause, except in the rheumatic cases. In the patients now under consideration, eight were under thirty years of age, and only three, or somewhat less than one-sixth, under twenty.

"Of the five rheumatic patients, three were under twenty years of age, and the remaining two, twenty-five each; this is satisfactory, for all experience in Europe proves that 'rheumatic pericarditis is peculiarly a disease of youth.'

"The particular orifices affected were as follows—viz. :

	Cases.
1. Aortic constriction	2
2. Mitral regurgitation	7
3. Mitral regurgitation and aortic constriction	2
4. Mitral and tricuspid regurgitation	3
5. Tricuspid regurgitation.	2
6. Seat of disease uncertain.	2

"These conclusions cannot be altogether depended on, as the evidence is founded chiefly on the physical signs; it is clear, however, from the table, that the mitral valves are individually the most liable to disease.

"Remains of former pericarditis existed in one case, in which the bag of the pericardium was completely obliterated by universal adhesion of the parietal to the visceral layer. And although the aortic valves were also slightly diseased, and the lungs emphysematous, the heart was neither dilated nor hypertrophied; on the contrary, it was decidedly smaller than natural; both kidneys were affected with fatty degeneration, this lesion inducing the fatal termination of the case. It may further be remarked that the kidneys were not diseased in the other cases examined after death."

5. *On Tubercular Disease in the East.* By Dr. WILSON. (*Indian Annals*, No. 3, p. 182.)

In the zillah jail at Rajshye, there was an average of 846 prisoners. The average annual mortality from tubercle was a little under 8 per 1000, or nearly the same rate as at home. Thus, in six years 165 men died, and in almost all, post-mortem examinations were made. Tubercles were present in one-fourth of the whole.

Dr. Wilson remarks :

"The natives of India form no exception to the dark races in other parts of the globe, or at least this much may be said, that the exemption from phthisis is by no means so universal as has been supposed, if portions of the continent of India are so exempt it is very desirable to have information regarding them, as convincing as that given of places where the disease has been ascertained. It is not surprising, that, previous to the universal practice of percussion and auscultation, the extent of the prevalence of the disease should have been overlooked, but at first sight it is surprising that it should not be universally recognised up to the present time, the explanation is found in the difficulty of always procuring post-mortem examinations, and in the peculiar and latent nature of the chest symptoms, and partly, no doubt, from education at home settling in the mind a belief to the contrary. . . .

"The disease, besides the common wasting form, which has procured for it the descriptive names of consumption and decline, shows itself in two varieties, more commonly than in Europe—the latent, and febrile; the latter is, possibly, often only a hurried termination of the latent. In the latent form, the chest symptoms may never appear, the disease being fatal by hurrying on of concomitant disease in the bowels, the effect of climate being to excite to activity the abdominal symptoms." (p. 188.)

6. *On Chlorosis simulating Phthisis.* By Dr. RILLIET. (*Archives Générales de Méd.*, Fév. 1855.)

AMONG the various forms of chlorosis is one little known, remarks M. RILLIET, which is accompanied by fever and grave functional derangement; it bears, in fact, a very close resemblance to phthisis. Thus, a young lady consulted M. Rilliet, with a diagnosis from her ordinary attendant that her lungs were profoundly diseased. The affection had commenced with dyspepsia, gastralgia, gradual diminution, and, at last, cessation of the catamenia, dyspnoea, pallor, and great prostration; then cough, with expectoration formed of saliva and greyish foetid sputa; then hæmoptysis came on, and speedily profuse sweats, rapid wasting, and the other more obvious symptoms of consumption. The physician in attendance declared that there were unequivocal signs of tubercles at the apex of the right lung. After learning these particulars, M. Rilliet thought it scarcely necessary to examine the chest in order to confirm the diagnosis. He did so, however, and, to his astonishment, was not able in any way to find the slightest physical evidence of tubercular deposition. He satisfied himself, in fact, that the physical signs must have existed only in the imagination of the medical attendant, who, too much impressed by the general state, had believed he heard that which would explain it. Unable to find anything in the lungs to account for the state, M. Rilliet examined the abdomen; here he found a tumour, over which he heard a placental murmur—in fact, the young lady was pregnant. This did not account for all the symptoms, however, but, on inquiry, he found the patient had been very chlorotic, and was evidently so, even at the time, to a considerable extent. He concluded, then, that the pregnant state, and the moral condition it necessitated, had combined with the general chlorotic condition to produce symptoms simulating so strongly those of phthisis. The result of the case completely confirmed the diagnosis.

In this instance, the existence complicated the case, but M. Rilliet goes on to refer to other simply chlorotic cases, with the same apparent symptoms of phthisis. When the case is recognised, it is necessary to disregard the cough, the night-sweats, &c., and to refer only to the cause, and for this cause iron is the remedy. A great objection exists in the minds of many physicians—Trousseau among the number—to the use of iron in tuberculosis. "Iron," says Trousseau, "augments, to a frightful extent, the accidents depending on tuberculous cachexia." Yet iron (with, of course, moderate exercise, and plenty of good air and food) is the only remedy for the simulated phthisis of chlorosis. Hence the necessity of an accurate diagnosis.

7. *On Ulceration of the Frænum of the Tongue in Whooping Cough.* By Dr. GAMBERINI. (*Annal. Univ. de Méd.*, 1854, and *Archives Gén. de Méd.*, Février, 1855.)

THE very frequent existence of little ulcers on the frænum of the tongue in whooping-cough has been noticed by two or three observers. Since 1844, Gamberini has looked for its presence in all the cases seen by him; he finds it generally, but not always, present, even in the most severe cases; the ulcer has seldom a round form; usually it lies transversely across, and cuts the frænum; it may be placed on the inferior surface of the tongue, near to, but not on, the frænum; it is never preceded by a vesicle, but commences at once as ulcer. Gamberini thinks this ulcer is produced mechanically, by the projection and laceration of the frænum against, and by, the teeth during the violent paroxysms of cough; it is in those cases in which the tongue during the cough is not carried against the teeth, but is retracted somewhat towards the pharynx, as sometimes happens, that the ulceration is absent. If the incisor teeth are of unequal height, the ulcer exists only, or is deepest, at the point where they project most. In one case in which the teeth had not appeared, there was no ulcer. In other cases of convulsive cough, Gamberini has not found the ulcer, but he does not regard this as militating against his explanation of its origin.

8. *On the Absence of Typhus in the Tropics, and in the Southern half of the Earth.*
By Dr. M——. (Henle's Zeitschrift für Rat. Med. 1854, Band v. p. 256.)

IN an interesting paper, this anonymous writer passes in review the various evidences found in writers which show that the European typhus is unknown, or almost so, in the tropics and in the southern hemisphere. He appears to be extremely well read in English literature, and draws many of his illustrations from English authors. He has not even omitted the late observations on the occurrence of three cases of typhoid fever in Burmah, by Mr. Scriven;* but he considers these to be doubtful. His conclusion is, that "Typhus is a disease only of the northern temperate zone, and that its southern limit is the isothermal line of 72° Fah.; it does not occur in the southern temperate zone, or at any rate it is not endemic there."

QUARTERLY REPORT ON MIDWIFERY.

By ROBERT BARNES, M.D. (Lond.)

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I. DISEASES OF THE SEXUAL SYSTEM.

1. *Observations on Painful Atrophies of the Mamma, Cirrhosis Mammæ, and Atrophic Sarcoma.* By Dr. A. WERNHER, of Giessen. (Zeitsch. für Rat. Med., Band v. Heft 1, 2. 1854.)
2. *On the Extirpation of the Uterus.* By REICHE, of Magdeburg. (Deutsche Klin. 43. 1854.)
3. *On Synostosis Sacro-iliaca in the Obliquely-contracted Pelvis.* By Dr. W. LAMBL. (Vierteljahrs. für e Prakt. Heilk., Band iv. 1854.)

1. THE essay of Dr. WERNHER is an excellent clinical contribution to the pathology of the female breast. We can only find space for some general conclusions. Dr. Wernher says he has seen many tumours of the breast which so closely coincided, in outward appearances, with scirrhus, that they would certainly have been declared scirrhus, and treated accordingly, but which minute microscopical examination showed to want all the elements of cancer, and to consist entirely of the tissue of the mamma in a peculiar atrophic condition. These observations induced him to examine a series of tumours in the Giessen Museum, part of which belonged to the collection of Socmmering, and described as "extirpated cancers with successful result." To his astonishment, he found in most of these the same appearances as those described above, as having occurred to him in recent cases.

The disease is never seen but in elderly women, who have passed the fortieth year. It was always seen in one breast only, the other being quite sound. The diseased breast did not differ much in size from the healthy one, sometimes it was less full. "Round the nipple there was an oval roundish tumour circumscribed. To the feel it appeared a hard knotty mass, which plainly included the whole glandular substance, but perhaps scarcely the fourth part of the circumference of the breast. In the remainder of the breast, at the edges and basis, nothing was found but fat. The diseased gland, as compared with the healthy one, was clearly contracted into a smaller space. The tumour was freely movable on the pectoral muscle, but closely united to the skin in the vicinity of the nipple. Here the skin was plainly thinned, free from fat, and its motion over the gland was completely lost. The colour was like that of the other breast. The nipple was harder, more knotty, and sometimes drawn inwards in funnel-shape. In one case, even the

axillary glands were swollen. The tumour was always very painful; the pains came on spontaneously by fits and starts, were shooting, worse and better according to the weather, and extending towards the axilla and arm of the diseased side. The general condition varied; in some it was altogether favourable, or only disordered through sleepless nights; in others, there were complications apparently unconnected with the affection of the breast. Microscopical investigation first freed the Author from his error that these tumours were scirrhus. (The microscopical appearances are figured, and several cases minutely detailed.) He is disposed to assimilate this condition of the breast to that known as cancer in the liver.

2. Dr. REICHEL states that he has extirpated the entire uterus seven times; in all cases the result was fatal. He, however, advocates the operation in cases of cancer confined to the organ. He then describes the method of operating; this presents nothing calling for analysis. Partial extirpation he represents as a painful operation, but one free from danger. It is indicated in all degenerations limited to the neck of the uterus.

3. The form of pelvic distortion described by Dr. LAMBL must not be confounded with the pelvis oblique ovata of Nagele. He refers to four cases of the affection recorded: the first by Robert, in 1811, which occurred to M. Dittmayer. The woman was 31 years old. The preparation is in the Würzburg Lying-in Hospital. The second by Kirchhoffer, of Altona. The woman was 21 years old. The preparation apparently in possession of Kirchhoffer. The third by Dr. Lambl. The woman 24 years old. The preparation in the Museum at Prague. The fourth by Robert, observed in Paris. The woman 17 years old. The wheel of a carriage going over her had injured the pelvis when six years old. The preparation in possession of Prof. Dubois. The cases yet known indicate an unfavourable prognosis for the mother, and even for the child. The mother died in all four cases; the Caesarean section was resorted to in three cases, namely, those of Robert and Kirchhoffer; cephalotripsy in one child, born dead in one, destroyed in one, living in two. Complete synostosis of the sacro-iliac synchondrosis is not an essential character of this deformity of the pelvis; osseous union was only partial in one, and complete in three. In all cases the oblique contraction depends upon narrowing of the sacrum, and stretching of the ossa innominata; these last are longer and higher than normal, and rise more perpendicularly. Only one of the pelvis is symmetrical. The dimensions of the pelvis from behind forwards are, in consequence of the stretching of the pelvis, not very different from the normal, and in the outlet are even somewhat greater; the oblique diameter, on the other hand, never reaches the normal, and at the outlet not even the half.

Dr. Lambl describes minutely the pelvis of his own case, but without the aid of engravings it would be difficult to give a sufficiently clear description. There was perfect bony union of both sacro-iliac articulations. The author considers that this synostosis was not of an inflammatory origin. He enumerates three kinds of causes of this distortion. 1. Mechanical violence, as fracture and dislocation. 2. Inflammation of the bones, and subsequent change of texture. 3. Abnormal development in relation to variations in size and form. The asymmetry is caused by the dislocation and abnormal position of the joint of the left os innominatum, and through the difference of its size from the right.

II. DISEASES OF FŒTUS.

On Fœtal Luxations, and especially on the Congenital Sub-coracoidal Luxation of the Humerus, &c. By Dr. A. MAYER, of Würzburg. (Verh. d. Phys. Med. Ges., Band v. Heft ii. 1854.)

THE luxations of the unborn child are obviously to be distinguished from those occasioned during labour, and call for a widely different treatment. Dr. MAYER gives a short historical retrospect of the first kind of luxations. He states that

during his own thirty years' orthopædic experience, 36 real foetal luxations have come before him.

a. Three were left shoulder dislocations forwards.

b. One complete symmetrical dislocation of both humeri at the elbow-joints in a man of 27.

c. One symmetrical luxation of the heads of both humeri backwards and outwards in a child three weeks old, who bore at the same time many other distortions.

d. Of luxations in the joints of the hand he has observed seven.

e. One incomplete luxation of the left patella outwards, which had remained unhealed eleven years.

f. One complete luxation of the left knee inwards, with secondary, acute-angled ankylosis of the tibia backwards.

g. Fourteen luxations of the hip-joint; of these, nine were symmetrical, five one-sided.

Dr. Mayer has seen but three cases of congenital dislocation of the shoulder-joint; all were on the left side; in all the head of the humerus was thrown forwards, under the coracoid-process, and a tolerably perfect new joint had formed on the anterior surface of the scapula. The first case was complicated with many defects of development. The child died of atelectasis in two hours. The second case was seen in a man 36 years old, who was content to bear with his atrophied arm. The third case was the son of a healthy man, but his mother had died, a few weeks after his birth, of caries of the spine and hectic. The patient was 24 years old. Before detailing this case, Dr. Mayer refers to the treatises of R. W. Smith, of Dublin, Melicher, Malgaigne, d'Outrepont, Dupuytren, and Ammon, upon similar luxations. In his own case he found the left shoulder almost atrophied to a skeleton. This atrophy extended to the arm, but in lesser degree to the fore-arm. There was but limited motion in the shoulder. The acromion projected considerably, and underneath, instead of the head of the humerus, the finger sunk into an empty cavity. The deltoid was so atrophied, that it could scarcely be made out. The head of the humerus lay under the coracoid process in an abnormal flat joint on the fore-part of the scapula, and on the ribs. Abduction and elevation of the arm impossible for the patient, but slight motion can be given by the surgeon. A minute comparison of the dimensions of the two arms showed that all the dimensions of the left arm were sensibly less than those of the right.

The causes of this luxation are predisposing, or occasional. These we pass over, as being chiefly hypothetical. The operation Dr. Mayer recommends for the relief of this deformity he calls *Osteotomy angularis partialis scapulæ*. He describes six stages in its performance. 1. Division of the skin and sub-scapularis. 2. Trepanning the spine of the scapulæ. 3. Sawing-out of the wedge of bone. 4. Sawing through between the coracoid process and the neck of the scapula. 5. Breaking the joint-pan, and removal of head of scapula. 6. Uniting the wound, and dressing. Although Dr. Mayer describes this operation as if it had been performed upon the living subject, it is not stated to have been performed in the case minutely detailed as to anatomical diagnosis; nor is anything said about observed results of the operation.

Injectons of Chloroform-vapour into the Uterine Cavity to Relieve Pain.

By M. ARAN. (Bull. de Théor., Jan., 1855.)

M. ARAN, extending the local application of the vapour of chloroform in uterine affections, recommended by Dr. Hardy, of Dublin, has adapted to Hardy's apparatus a hollow uterine sound, pierced at the end by two openings; this is passed into the uterine cavity. Caution is advised not to inject the vapour too suddenly, lest the uterus be distended; but done gradually, it is said that instant relief is given to uterine pain. Five cases are reported: in three the effect was favour-

able; in one of these, a case of post-*puerperal* metritis, pain was completely suspended, and on a second injection altogether stopped; in a second, a case of chronic metritis, with an irritable condition of the uterus, two injections produced a permanent amelioration; in the third, a case of retroflexion, in which the intra-uterine pessary could not be borne, after a few injections the instrument could be worn for several days at a time. In the two other cases, the effect was not so marked: in one, of retroflexion with chronic inflammation, the injections at first caused great pain, it is supposed from being forced too rapidly; but relief followed; in the other, of obstinate dysmenorrhœa with colics and nervous phenomena, relief was but momentary, whilst the injection of a few drops of laudanum into the uterine cavity gave ease which lasted for twenty-four hours.

III. LABOUR.

1. *Observations on the Temperature of Lying-in Women.* By C. HECKER. (Annal. du Charité-Krankenhaus zu Berlin. 5 Jahrg. 2 Heft, 1854.)
2. *Induction of Premature Labour on Account of Eclampsia by the Colpeurynter.* By SCHILLINGER. (Schmidt's Jahrb., 1855, No. 27.)
3. *Case of Rupture of the Uterus; Gastrotomy; Recovery.* By JOHN K. MASON, M.D. (New York Journal of Medicine, Jan. 1855.)
4. *Cæsarean Section ending Fatally.* By Dr. LA ROCHE. (Med. Zeitung, 13 Dec., 1854.)
5. *Cæsarean Section Twice successfully Performed on the same Subject.* By W. H. MERINAR, M.D. Misso. (New York Journal of Medicine, Jan. 1855.)
6. *Cæsarean Section in a Rachitic Woman.* By Prof. DUBOIS. (Gaz. des Hôp., 6 Fév., 1855.)

1. THE observations of Dr. HECKER embrace an inquiry into the temperature of the body in lying-in women at different periods, from immediately after delivery down to the end of the *puerperal* state. The results arrived at do not appear to be very definite; they must be accepted with caution. The following are the principal conclusions:

1. In 35 cases, the thermometer was placed in the vagina immediately after delivery, this being in all cases natural. In many of these cases, the thermometer indicated a marked increase of heat. This bore no constant relation to the duration of the whole labour, or of the expulsive stage, but rather seemed to depend upon the intensity and rapid succession of the pains.

2. In the first stage of the *puerperal* state, a sinking of the thermometer was observed; this was the more remarkable in proportion to the elevation of the temperature immediately after delivery. On an average, the thermometer stood at the lowest point twenty-four hours after delivery.

3. After this time there was, in the minority of the cases, an even temperature, interrupted only by evening exacerbations and morning remissions. In most cases, the production of heat rose considerably.

4. This elevation of heat was connected with a decided increase in the frequency of the pulse; it bore no constant relation to the condition of the breasts; it was only when it attained a certain degree, that a reaction of the whole system was observed in the form of milk-fever.

5. The period when the thermometer reached its greatest height was very various. On an average, it was seventy-seven hours after delivery.

6. The rise of temperature appeared to be less frequent amongst pluriparæ than primiparæ.

7. The same was also observed in lying-in women who did not suckle their infants.

8. After the maximum temperature, a fall was observed, as in those diseases which determine through lysis; less frequently, there was a fall like that in the

critical determination of diseases; later, a stage of inanition was observable by means of the thermometer.

9. Puerperal diseases are not adapted to thermometrical studies; it is only exceptionally that useful indications can be drawn from observations relating to temperature.

2. A strongly-built primipara, aged 20, fell into convulsions, with loss of consciousness, in her sixth month; the urine contained a great quantity of albumen; the legs and abdomen were cedematous. Cold affusions to head, mustard cataplasms, chloroform, were useless. To bring on labour, India-rubber bladders were applied to the breasts, and at the same time, the colpeuryuter to the uterus; besides this, the os uteri was dilated by the finger. The most violent convulsions during the operation were moderated by chloroform. The child breathed some hours. Consciousness returned on the birth of the child. The albuminous character of the urine lasted, gradually lessening, till the fourteenth day. The woman recovered slowly.

3. Dr. Mason's patient was in labour with her sixth child. Rupture of the uterus occurred in the act of defecating; immediately on its occurrence, she complained of intense, agonizing, burning pain in the right side. The head of the child receded, and could be felt through the abdominal parietes. Twelve hours after rupture, Dr. Neil performed gastrotomy. The child, which was hydrocephalic, was removed, together with the placenta and large quantities of coagula. In one month, the patient had quite recovered.

4. The patient was twenty-eight years old, of middle stature, well-nourished, and well-built. She had never suffered from rachitis or osteo-malacia. On examination during labour, the pains having ceased, and the liquor amnii having escaped six hours, a growth from the promontory was felt, projecting so as to contract the anteroposterior diameter to two and a half inches. The midwife had mistaken this growth for a second head. Only a small portion of the living head had penetrated into the pelvis. The patient had as yet suffered so little from her labour, that she was in full strength. Cæsarian section was determined upon. The use of chloroform had no effect in depriving her of sensation, and was given up. A strong living child was withdrawn with some trouble, the wound in the uterus being made too small in relation to bulk of child. Hæmorrhage followed extraction of child, but was arrested by speedy separation of placenta, which adhered to the posterior wall. The patient seemed cheerful for the first two days; the lochia flowed; skin cool; pulse small, frequent, and hard; hiccough. Severe pain on third day. Thirty leeches applied to abdomen; calomel and morphia; hiccough subsided. After apparent amendment, she died on the fifth day. Autopsy refused by relations.

5. Dr. MERINAR's case is that of a woman aged twenty-four, who was taken in labour on the 14th of July, 1852. There was great deformity of pelvis; its anteroposterior diameter not exceeding two inches. Irritative fever set in, and great tenderness over abdomen. Gastrotomy was performed, and a dead male child extracted. No very severe symptoms followed; and on the 20th of September she had entirely recovered. On the 22nd of May, 1854, she was again in labour. The same conditions rendered gastrotomy again necessary. The incision was made parallel with the first; and a living male child removed. But little hæmorrhage. On the fourth day after the operation somewhat severe symptoms set in: but on the 28th of August both mother and child were enjoying good health.

6. The woman who was the subject of Cæsarean section by M. DUBOIS came into the Hôpital des Cliniques on the 25th of January. She was thirty-two years old, rachitic, and a primipara. Very short, only one millimetre sixteen centimetres. There was œdema of the legs, and albuminuria. M. Dubois has observed that

eclampsia is more common in the rachitic than in others, but observes that in these also it might have been connected with albuminuria, which had not been noticed. The patient was at the full time on admission, and in labour. The narrowness of pelvis ascertained, M. Dubois decided in favour of Cæsarean section, without attempting cephalotripsy. On the 26th the incisions were made under chloroform. The child was extracted alive. The extraction of the placenta was followed by considerable hæmorrhage. The chloroform had annulled the consciousness of the pain of the operation, but not the reality. She knew nothing of it, but the agitation and excitation manifested prove that she was in pain throughout the operation. She took a small dose of opium at night. A knuckle of intestine made way out of abdomen, and being much inflated with gas, punctures were made in order to reduce it. Vomiting came on next day, fainting, and she died. The autopsy:—No trace of inflammation or internal hæmorrhage; only a small clot in the iliac fossa. Pelvis: antero-pubic diameter fifty-four millimetres. The symphysis pubis was completely ossified, and projected into pelvis; the transverse diameter was twelve centimetres, and the two oblique eleven centimetres.

MEDICAL INTELLIGENCE.

Nurses for the Poor.

THE unremitting exertions of Dr. Sieveking to accomplish the twofold object of providing nurses for the poor, and of finding in this way appropriate occupation for the unemployed women of the labouring classes, are being apparently crowned with success. A committee of the Epidemiological Society have ascertained that there are in the various workhouses of England a large body of unemployed able-bodied women (13,352 altogether, of whom 5,634 are of good character), who might very readily be trained to act as nurses. The committee propose—

I. That the master and matron of every workhouse shall give such female inmates a routine of occupation that shall afford them a knowledge of the duties required in the management of the sick.

To carry out the plan proposed it would be requisite, that a general order of the Poor-Law Board be issued to every board of guardians, directing this, and the following provisions, to be enforced in their respective workhouses. This plan would entail no organic change in the classification and management of the inmates. Each female on being admitted would be put to the employment for which she appeared best fitted. After receiving the necessary instruction in the culinary and domestic department, she would be transferred to the infirmary; where, under the superintendence of the matron, nurse, and medical officer, she would be able to acquire a proper knowledge of the duties of nurse.

II. That the medical officer of each workhouse, as soon as he shall consider an inmate competent to undertake the nursing of the sick out of the workhouse, shall certify to that effect.

The master and matron of the workhouse would regulate the matters of detail with regard to the earlier stages of the training, and judge of those whose behaviour, character, and general aptitude would qualify them to be trusted to attend upon the sick. The medical officer would determine the character of the certificate, and the period when it should be given.

III. That a register shall be kept at the workhouse of all those who have been certified by the medical officer as qualified nurses.

The registers would, collectively, form a source from which nurses might be selected, not only for private individuals, but also for public institutions. A means of livelihood would thus be opened to the workhouse inmate, and her position would be raised.

This plan appears so simple, and at the same time so likely to be of the greatest use both to the nurses and to the nursed, that we trust the Epidemiological Society may be successful in obtaining for it the sanction of the Government.

The Examination for the East India Company's Medical Service.

The late examination of candidates for commissions as assistant-surgeons in the East India Company's Service, heralds a new era in the public departments. It cannot now be long before all appointments held under government will become the rewards of industry and talent, and will no longer be the usurped property of rank and wealth, or the means by which the government of the country buys the support of the representatives of the people. To our own profession it is impossible to overrate the importance of the new system, both in giving a stimulus to medical education in this country, and to the cultivation of science in India.

The number of candidates at the last examination was limited, on account principally of the drain of the younger medical men to the Crimea. The same cause will doubtless operate more or less till peace be declared, or till the increased number of entries into the profession somewhat restore the balance between the supply and demand. Eventually, however, we entertain no doubt that the competition for the Company's appointments will be very great.

We subjoin the substance of the Report made by the Examiners to the President of the Board of Control, and the names of the successful candidates, arranged in order of merit.

"The examination commenced on the 8th, and terminated on the 11th of January. On Monday and Tuesday, January 8th and 9th, the candidates were examined, in writing, in medicine, surgery, anatomy, and physiology, and natural history. Three hours were allotted to each subject. Copies of the questions are annexed to this letter. On Wednesday we proceeded to the oral examination. Each candidate was questioned on the various subjects for an hour. Thursday, January 11th, was occupied with practical examinations in medicine and surgery. These were conducted at University College Hospital, where convenient rooms had been placed at our disposal by the authorities. Here were assembled from various sources (not from the wards of the hospital) a number of patients with well-marked medical and surgical complaints. Each candidate was called upon to examine a medical and a surgical case, and then to write briefly his opinion of their nature, and the plan of treatment he would adopt. The examiners in surgery and in anatomy then tested the candidates in operations on the dead body; one capital and one minor operation being assigned by lot to each candidate."

Names of the successful Candidates.

- | | |
|--------------------------------------|---|
| 1. Marr, George, M.D., L.R.C.S.E. | 12. Niven, Wm., M.D., M.R.C.S. Ed. |
| 2. Chuckerbutty, S. C. G., M.D. | 13. Lowe, Thomas, M.R.C.S. Eng. |
| 3. Brake, John, M.R.C.S. Eng. | 14. Cook, Henry, M.R.C.S. Eng. |
| 4. Hill, Marcus G., M.R.C.S. Eng. | 15. Dick, Robert, M.D., M.R.C.S. |
| 5. Martell, Alfred A., M.R.C.S. Eng. | 16. Atkins, Henry, M.R.C.S. Eng. |
| 6. Collison, John B., M.R.C.S. Eng. | 17. Webster, Henry, M.R.C.S. Eng. |
| 7. King, John B., M.R.C.S. Eng. | 18. Watson, Richard, L.R.C.S. I. |
| 8. Morgan, Arthur, L.R.C.S. I. | 19. Dunman, George, M.R.C.S. Eng. |
| 9. Brown, James, M.R.C.S. Ed. | 20. Drew, Charles, M.R.C.S. Eng. |
| 10. Doyle, William, F.R.C.S. I. | 21. Falconer, D. M., M.D., M.R.C.S. Ed. |
| 11. Lloyd, Edmund E., M.R.C.S. Eng. | 22. Foy, James A., M.R.C.S. Eng. |

The printed questions may be seen in the parliamentary return of the copy of the Report, ordered to be printed, 29th January, 1855; they have also been published in the January numbers of the 'Medical Times and Gazette.'

BOOKS RECEIVED FOR REVIEW.

Unsoundness of Mind in Relation to Criminal Acts. (Essay to which the first Sugden Prize was awarded.) By J. C. Bucknill, M.D. Lond. London, 1854.

Report on the Mortality and Public Health of Oxford during the year 1854. (With an Appendix on the Social and Sanitary Condition.) Oxford, 1854.

Untersuchungen und Experimente als Beitrag zur Pathogenese des Icterus, und der Acuten gelben Atrophie der Leber. Von Dr. Th. von Dusch. Leipzig, 1854.

On the Nature, Signs, and Treatment of Childbed Fever. By C. H. Meigs, M.D. Philadelphia, 1854.

On the Construction and Organization of Hospitals for the Insane. By F. S. Kirkbride, M.D. Philadelphia, 1854.

The Practitioner's Pharmacopœia and Universal Formulary. By John Foote, M.R.C.S.E. London, 1855.

«Eukierapœia; or, an Examination of the Principles of Medical Science. By Robert Garner, Surgeon to the North Staffordshire Infirmary, &c. London, 1855.

Notes on some of the Developmental and Functional Relations of certain portions of the Cranium. Selected by F. W. Pavy, M.D., from the Lectures on Anatomy by John Hilton, F.R.S. London, 1855.

Rules of the Society for Improving the Condition of the Insane; and the Prize Essay on the Changes which have taken place since the time of Pinel in the Moral Management of the Insane. By Daniel H. Tucker, M.D. London, 1854.

Transactions of the Belfast Clinical and Pathological Society for the Session 1853-54. Belfast, 1854.

Pharmacopœia Norvegica. (Regiâ auctoritate edita.) Christiania, 1854.

The Diagnosis of Surgical Cancer. (The Liston Prize Essay for 1854.) By John Zachariah Laurence, Surgeon to the Northern and Farringdon Dispensaries. London, 1855.

On the Mode of Communication of Cholera. By John Snow, M.D. Second Edition. London, 1855.

De la Longévité Humaine et de la Quantité de Vie sur la Globe. Par P. Flourens. Paris, 1854.

The Retrospect of Medicine. By W. Braithwaite. Vol. xxx. July—December, 1854.

The Half-Yearly Abstract of the Medical Sciences. By W. H. Ranking, M.D. Cantab., and C. B. Radcliffe, M.D. Lond. Vol. xx. July—December, 1854.

Chloroform; its Properties and Safety in Childbirth. By E. W. Murphy, M.D., A.M. London, 1855.

Geschichte der Forschungen über den Geburtsmechanismus. Von Gustav Knoes. (Inaug. Diss.) Gießen, 1854.

Die Galvanocaustik ein Beitrag zur Operativen Medicin. Von Dr. A. T. Middeldorp. Breslau, 1854.

Medical Jurisprudence. By Alfred S. Taylor, M.D., F.R.S. Fifth Edition. London, 1854.

Food and its Adulterations. By Arthur Hill Hassall, M.D., Comprising the Reports of the Analytical Sanitary Commission of the Localities.

Elementary Treatise on Chemistry. By William Gregory, M.D. Edinb. 1855.

The Pathology of Drunkenness. By Charles Wilson, M.D. Edinb. 1855.

Handbuch der Speciellen Pathologie und Therapie. Redigirt von Rud. Virchow, Professor der Medicin in Würzburg. (Five additional Parts.)

Schödel Kinn und Felle des Menschen und der Thiere. Von Emil Huschke. Jena, 1854.

The Micrographic Dictionary. By J. W. Griffith, M.D., F.L.S., and Arthur Henfrey, F.R.S., F.L.S. Parts VI., VII.

Ueber Blutungen aus der Nabelschnur und dem Nabel. Von Joseph Taurelle. (Inaug. Diss.) Gießen, 1854.

Die Pathologie und Therapie der Gehirn Krankheiten. Von Dr. Rud. Leubuscher. Zweite Abtheilung. Berlin, 1854.

Lithotomy Simplified, or, a New Method of Operating for Stone in the Bladder. By George Allart, M.R.C.S. London, 1854.

The Pathology of the Bronchio-pulmonary Mucous Membrane. By C. Black, M.D. Edinburgh, 1855.

Archives de Physiologie, de Thérapeutique, et d'Hygiène. Par M. Bouchardat. No. 2. Octobre, 1854. Sur l'Action Physiologique et Thérapeutique des Ferrugineux. Par T. A. Quévenne. Paris, 1854.

A Clinical Report on the Cases of Pneumonia treated in the Clinical Ward of the Jarnetjee Jejeebhoy Hospital, from 1818 to 1853. By C. A. Morehead, M.D. (Reprint from the Bombay Medical Transactions.)

Specielle Heilmittellehre. Von Dr. W. J. A. Werber. Band II. Zweite Abtheilung.

Iconographie Ophthalmologique. Par J. Sichel. Livre V., VI., VII. Paris, 1852.

A Practical Treatise on Foreign Bodies in the Air Passages. By S. D. Gross, M.D. Philadelphia, 1854.

Traité d'Anatomie Pathologique, Générale et Spéciale, ou Description et Iconographie Pathologique. Par le Dr. Lebert, Professeur de Clinique Méd. à l'Université de Zurich. Livraisons 1 and 2.

Practical Treatise on the Diseases of Children and Infants at the Breast. Translated from the French of M. Bouchut; with Notes and Additions by Peter Hinckes Bird, F.R.C.S. London, 1855.

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OR

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VOL. XV.

JANUARY—APRIL 1855.

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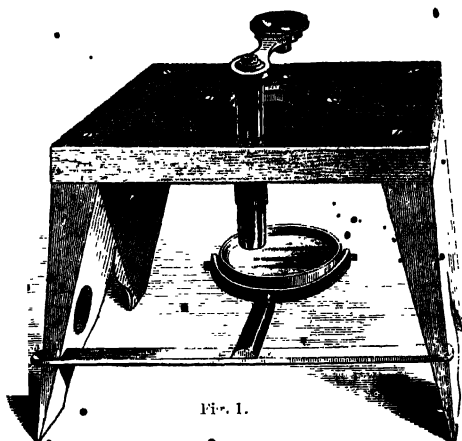


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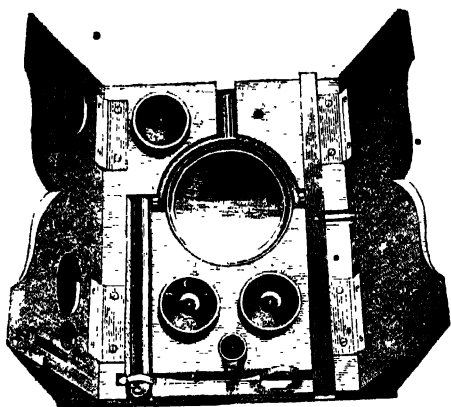


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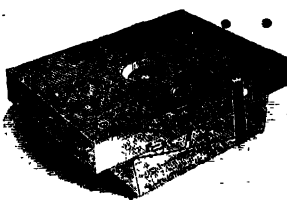
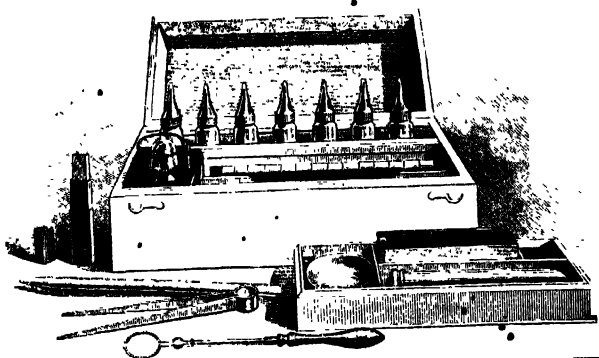


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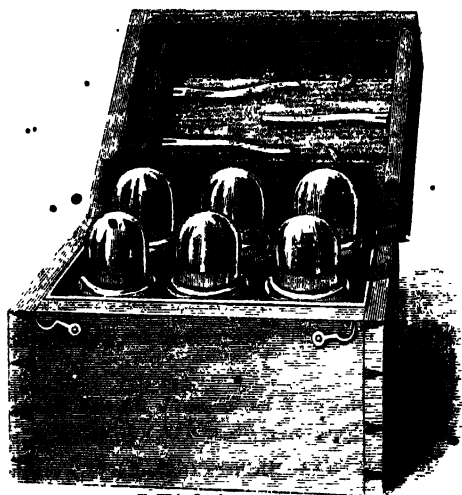
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